THE FILLED PAUSE AND SOCIAL ASPECTS OF CONVERSATIONS

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BY

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SUBMITTED IN FULFILMENT OF THE REQUIREMENTS FOR THE DEGREE OF
MASTER OF ARTS

UNIVERSITY OF TASMANIA
HOBART

JANUARY, 1974.

I declare that this thesis contains no material which has been accepted for the award of any other degree or diploma in any university and that, to the best of my knowledge and belief, no copy or paraphrase is contained herein of material previously published or written by any other person, except where duly acknowledged in my text.

Peter J. Ball
31st January, 1974.

ACKNOWLEDGEMENTS

I am grateful to Professors George Seth and James A. Cardno, respective supervisors of the earlier and later stages of this work, for encouragement and guidance; to my former colleague Ken Macrae for valuable early suggestions and criticisms; to my wife Sue for help in recording and transcribing; to the Tasmanian students who acted as experimenters and recorders in Experiment IV; to Judy Howard for last-minute calculating and checking; to Paula Nash for meticulous arrangement of the typescript and spotting passages of nonsense; and to the numerous subjects who participated in my experiments.

P.J. Ball

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ABSTRACT

Selected developments in the fields of verbal and nonverbal behaviour are reviewed, with especial reference to the study of speech in social context. Among features bordering on both language studies and social psychology are hesitations and disruptions in speech. Silent pauses have been found to result from need to plan verbal sequences and a miscellaneous group of speech disruptions known as NonAhs is a sign of topical anxiety.

Filled pauses ('er', 'um' and variants, also called Ahs) were first thought to belong with NonAhs but have proved unrelated to anxiety and require a separate explanation. One hypothesis, that they have an interpersonal role in apportioning the conversational floor, has fared inconclusively under test and recent writers have written it off.

In two experiments, filled pause rate was measured as a dependent variable. Mutual visibility in dyadic conversations was varied from zero through intermediate levels to normal, but no changes were observed. When an interviewer's tendency to interrupt was varied, again no significant differences in Ah rate were recorded. However, the filled pause as an independent variable elicited effects supportive of the floor control hypothesis.

'Matched guise' recordings of a speaker were heard by independent groups of undergraduates and presence of Ahs yielded ratings of speaker anxiety, caution and submissiveness, consistent with either the discredited anxiety hypothesis or that of floor control. In a final experiment, naive subjects each interviewed a person whose answers varied in grammatical completion and whether they terminated with Ahs. Either grammatical incompletion, an Ah or both prolonged latencies of subjects' next question substantially.

The view that conversations are competitions for the floor is rejected for a broader outlook on interpersonal regulatory cues. Ahs probably do act in floor control, but less simply than previously thought and they may have other, non-regulatory roles besides.

Avenues for future research on the topic are outlined with methodological suggestions and the work is presented within a suggested systematisation of accumulating knowledge about interrelationships between linguistic features at several structural levels and various aspects of social behaviour.

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The first man I saw was of a meagre aspect with sooty hands and face, his hair and beard long, ragged and singed in several places. His clothes, shirt, and skin were all of the same colour. He had been eight years upon a project for extracting sunbeams out of cucumbers, which were to be put into vials hermetically sealed, and let out to warm the air in inclement summers

Swift, Gulliver's Travels: Part III, A Voyage to Laputa, Chapter 5 (on the Academy of Lagado).

THE FILLED PAUSE AND SOCIAL ASPECTS OF CONVERSATIONS

CHAPTER ONE

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LANGUAGE & SOCIAL BEHAVIOUR

This thesis is concerned with a restricted aspect of talking, peripheral to both psycholinguistics and social psychology and the author had best state at the outset how this topic should have attracted him. The behaviour studied is the Filled Pause, i.e., 'er', 'ah', 'um', 'uh' and variants, also referred to in the scientific literature as '<u>Ah</u> speech disruptions'. Both terms will be used to denote the phenomenon in the present report, as well as occasionally the abbreviation, FP.

As a topic, 'ers', 'ahs' and 'ums' smacks of triviality, but filled pauses are uttered with astonishing regularity, and by some people more than others. Under suitable conditions rates as high as a filled pause for every ten words often occur, which can mean intervals averaging only three or four seconds. They have no denotative meaning, if they can be said to have any meaning at all, so it is worthwhile to ask why they are produced. Because they appear to have no meaning in the normal sense of the term, and because they are not constrained by grammatical rules, they are not language, yet they are utterances in a sense that coughs and hiccoughs are not. Cook (1968), having failed to explain them to his own satisfaction, asked whether they might be after all'...mysterious and inexplicable phenomena...' but, if their significance is not transparent, it would be well to investigate them and seek it out. This writer's interest in the filled pause stems mainly from the fact that he became interested in language as a social phenomenon and found that these 'mysterious and inexplicable phenomena' pose a puzzle precisely where speech and social behaviour interlock.

'Sociolinguistics' as a name for the study of language and social behaviour has become widespread and, although trite, is shorter than 'Social psychology of language' and more lucid than 'Ethnography of communication', so it is the word which will be used hereunder, whenever reference is made to the study of speech in its social context. The 'field' is not really a field at all, in the way that Perception or Motivation are fields, but a stamping ground for social scientists of many kinds, linguists and educationalists, who like crossing the boundaries of their disciplines. Hymes (1972) put it thus:

> *It is not necessary to think of sociolinguistics as a novel discipline. If linguistics comes to accept fully the sociocultural dimensions, social science the linguistic dimensions, of their subject matters and theoretical bases, sociolinguistic will simply identify a mode of research in adjacent sectors of each. As disciplines, one will speak simply of linguistics, anthropology, and the like...But, as just implied, the linguistics, anthropology, etc., of which one speaks will have changed. In order to develop models, or theories, of the interaction of language and social life, there must be adequate descriptions of that interaction, and such descriptions call for an approach that partly links, but partly cuts across, partly builds between the ordinary practices of the disciplines. This is what makes sociolinguistics exciting and necessary. (p.41)*

On the one hand, sociolinguistics may be 'exciting and necessary', because it cuts across conventional boundaries of disciplines, but it also lacks, for the same reason, an established general frame of reference or 'paradigm' (Kuhn, 1962) and this is reflected in paucity of textbooks. Such a field is not easy to acquaint oneself with quickly and the recent publication of two good introductions by Fishman (1970) and Robinson (1972), with more rumoured to be in press, augurs better for the aspiring student. In the meantime, there remains, and may continue to remain, the freedom to

choose topics, tools and theories without normal constraints of disciplinary propriety and it is possible to devise methods and terminology to suit the job, with free rein for eclecticism, in those who possess it.

When first introduced to the psychology of language, as an undergraduate, the writer had been recommended to study the field as a good second best to a course on social psychology which was temporarily unavailable and he was immediately gripped to read, in some of the psycholinguistic texts of the time, that language was inextricably bound up with the complexity of human social relations, in its uniqueness as a means of communication. Such a statement now sounds passe, but what was implied at the time seemed to be that a new vista was on the point of opening up, a matrix of linguistic and social dimensions which would reveal unforeseen aspects of the nature of Man. Most undergraduates have these experiences but the author makes no apology for his naivety, since he still believes the perception to be, at least partly, genuine. But, whereas writers in the nineteen-fifties and early sixties opined confidently that sociolinguistics (they did not use the term, at the time) would yield an exciting expansion of knowledge, they were much less sure how this was to be accomplished.

Since that time, the picture has cleared a little, largely because, as seems easier to accept now, whatever is to be learned in the field will emerge as a result of unostentatious research, using methods well-tried on the whole, if novel to some of the persons who use them, a fact which may have been apparent to some at the beginning. The magic matrix has not been unveiled in a sudden flourish by a grand sociolinguistic theorist, nor will it. The fields of linguistics proper and the psychology of social behaviour have moved ahead, with

significant advances in both, but without much interdependence, on the whole. Researchers in both areas are aware of each other's existence, but there has not been a great deal of theoretical integration, although there is a search for opportunities to promote this. No lack of success which might reasonably have been expected is implied because it is simply a reflection of the facts which have been discovered. However, in attempting to relate one thing to another, in the pages which follow, the writer has necessarily had to be his own guide and so many of the interpretations in this chapter are tentative.

In the next three sections will be outlined some developments in psycholinguistics and social psychology which the writer thinks relevant, although some of them only indirectly so, to his own particular work and an assessment of work in sociolinguistics.

1.1 PSYCHOLINGUISTICS

1.1.1 Linguistics before 1960

The study of language in the Western World appears to be divisible into three stages. The first lasted through most of the Christian era up to the twentieth century, and was characterised by the application of Classical grammar to the emerging modern languages of Europe, a relatively straightforward practice, since most were in direct line of descent from Latin and Greek. One of many side-effects from the colonisation of the Americas, however, was to bring white men into close contact with non-Indoeuropean tongues, and unwritten ones at that (unlike the Semitic languages which had, of course, been familiar to scholars throughout the first period). American linguists, in particular Franz Boas and his pupils, found that Classical categories

could not accommodate the structures they encountered. The urgency of recording languages before they became extinct probably assisted in abandoning Latin and Greek as ultimate Courts of Appeal and a strictly empiricist approach developed, which involved minimum pre-suppositions about the nature of Language. Discovery procedures for the observing, recording and analysis of exotic languages were the preoccupation of linguists in America, who were very much applied scientists with practical goals in mind, often aiming to save the souls of speech-communities studied, often concerned to promote assimilation to European material culture with its medicine and technology. The linguist was also frequently trained in anthropology, observed as a participant and tried to see language in its cultural context.

During this second period of Western linguistics, although there were certainly some psychologists interested in language, and probably more than is now generally appreciated, not very much influence of linguistics upon psychology really was felt. Linguists, led, during the nineteen-twenties, thirties and forties, by Bloomfield, adopted, and adapted to suit their needs, some of the philosophy of Watsonian Behaviourism. They attempted to observe actual speech, as uttered, which was less widespread before, but they relied strongly upon questioning native-speaker informants to test the grammars they were constructing and this might have struck behaviouristic psychologists as dependence upon subjective sources of data. In fact, the discovery procedures of Bloomfieldian linguistics could yet prove to be relevant to other disciplines, including psychology, which have not so far shown appreciation. The popularity of 'Direct Method' teaching of foreign languages, during part of the period, may be seen as an effect of

behaviouristic linguistics upon education and perhaps educational psychology. It is interesting to note that the technique is intended to give the learner a general communicative competence including skills not strictly linguistic, whereas alternative, more formal, approaches develop a narrower, linguistic competence which may be accompanied by extreme gaucheness in actual conversation. The latter kind of competence would be closer to the linguistic competence of which Chomsky writes (cf. 1.1.2).

During the last fifteen years of the second phase of Western linguistics, psychologists started to take an active interest in language, partly because of the impetus in communications engineering which resulted from the Second World War. An important textbook was published by George Miller in 1951, which is still interesting to read as an example of broadly behaviourist psychology attempting to come to grips with phenomena to which its traditional terms cannot convincingly be applied. The concentration is upon statistical distributions of words; child speech is dealt with mainly in terms of vocabulary acquisition and retardation; there are two chapters on verbal habits; syntax is mentioned twice. It is not easy for a contemporary student to imagine, without going back to books like Miller's, how language appeared to a psychologist in 1950, or even in 1955, such has been the impact of linguistic developments since then.

After the publication of Miller's book, psychologists found a few years' inspiration in the mathematical theory of communication of Shannon and Weaver (1949), which offered methods for quantifying transmitted information. The value of this theory, and the research psychologists pursued under its influence is easily underestimated today, but it led some to the absurd assumption that grammatical rules

were merely shorthand for statistical distributions; that there was no difference between uncommon utterances and ungrammatical ones. This was not discouraged by behaviourist linguistics, which, though it used prescriptive rules in practice, claimed to provide descriptions of actual speech, as it occurred.

The year 1957 saw the publication of three rather important books in the psychology of language. Osgood, Suci and Tannenbaum had written a monograph on The Measurement of Meaning, in which they set out a semantic theory, based upon Hullian learning theory concepts, and reported the development of a technique for assessing meanings. From the Harvard operant conditioning laboratories came Skinner's interpretation of Verbal Behaviour and Chomsky, of the Massachusetts Institute of Technology, published his first major opus, Syntactic Structures. The following year, Brown's Words and Things, which offered a cognitive psychological view of language, was published. It is probably fortunate that Brown's book was published too soon to make serious mention of the other authors' work, particularly that of Chomsky, since it now provides, unsullied, an account of language by a psychologist of that time who was not open to the more extreme criticisms now levelled at pre-transformational psycholinguistics and shows that the field was less bankrupt than detractors have maintained.

1.1.2 Contemporary psycholinguistics

Osgood's mediational representation theory of language had been outlined, in its main points, in an earlier book (Osgood, 1953) and has since been elaborated further (Osgood, 1963). It views meanings as fractional representational mediating responses ($\underline{r}_{\underline{m}}\underline{s}$), conditioned to Sign Stimuli through contiguity with other stimuli (Significates),

which normally elicit responses of which the $\underline{r}_{\underline{m}}$s are component parts. A motor-sensory feedback stimulus, $\underline{s}_{\underline{m}}$, triggered off automatically by the $\underline{r}_{\underline{m}}$, completes a two-stage mediational process which evokes a linguistic response. Criticism of the theory has centred upon whether the two stages of the mediation process are operationally distinct from a single stage account which is widely accepted as inadequate, by behaviourists and non-behaviourists alike. Osgood's theory represents an outcome (though not the only such outcome) of applying Hullian concepts to language and has retained some respectability among psychologists attached to S-R traditions. Skinner, by contrast, has fared less well.

Skinner proposed a 'Functional Analysis of Verbal Behaviour', arguing that established ways of tackling language, which used such terms as 'meaning' and constructs as grammatical rules to explain our speech and writing, terms which refer to objectively unobservable phenomena, must cloud issues which would be settled only by ascertaining the events in a speaker or hearer's environment that <u>control</u> his utterances or responses to utterances. In stating this, Skinner was no more than applying, in the verbal sphere, principles he had promulgated consistently over many years in the spheres of animal and human non-verbal behaviour. The rest of his book detailed how the parsimonious tenets of operant conditioning could be applied to verbal behaviour in an illuminating fashion, the reinforcement contingencies for a number of classes of verbal response being treated in some detail. Skinner was deeply interested in how speech acts were related to surrounding situational contexts: he offered <u>mands</u>, which generically covered commands, demands, requests, etc.; <u>tacts</u>,

which included instances of naming of objects and the like; echoic responses, or repetitions of utterances heard, and other, more subtle categories, suggesting how each was selectively reinforced by the language community.

Skinner's proposals met with criticism from many quarters, but none of it so influential as that of Chomsky (1959), described by Ingram (1968) as a 'savage and largely irrelevant review' (p. 320). Chomsky maintained that after eschewing unobservable postulates Skinner went on to invent his own which were equally unobservable, that he extrapolated unjustifiably to language from highly controlled laboratory experiments on simple aspects of small animal behaviour, that his definition of verbal behaviour does not exclude a variety of non-verbal behaviours and that his theory does not account for any verbal behaviour at all. The classes of utterance in Skinner's catalogue were considered in turn and Chomsky pointed out their absurdity to his own, if not everyone's, satisfaction. He concluded:

> *At any rate, just as the attempt to eliminate the contribution of the speaker leads to a 'mentalistic' descriptive system that succeeds only in blurring important traditional distinctions, a refusal to study the contribution of the child to language learning permits only a superficial account of language acquisition, with a vast and unanalyzed contribution attributed to a step called 'generalization' which in fact includes just about everything of interest in this process. If the study of language is limited in these ways, it seems inevitable that major aspects of verbal behaviour will remain a mystery.*

Commenting upon his review eight years later Chomsky (1967), stated that his views had not changed substantially on the subject and, in a barbed compliment, that he had chosen to attack Skinner's position so thoroughly because he regarded it as 'the most careful

and thoroughgoing presentation of such (i.e. behaviourist) speculations'. His endeavour had been to demonstrate that Verbal Behaviour could be regarded as a reductio ad absurdum of the empiricist position on language. Others might prefer to believe that Chomsky selected Skinner for his target on grounds of vulnerability and his linguistic naivety, but there is no doubt that Chomsky intended his criticisms to extend to behaviouristic treatments less esoteric than that of the operant conditioning school.

Although the present writer would not deny that Skinner neglected, ignored and denied issues of real import in his book, it is also true that Chomsky's treatment of it was, in parts, superficial and that he resorted to 'Straw Man' arguments to discredit his colleague. To take a well-known example, Chomsky devoted some paragraphs to drive-reduction conceptions of reinforcement, with the implication that his criticisms of these were relevant to Skinner's notion of reinforcement, when he must have known that Skinner had always repudiated the drive-reduction formulation. Wiest (1967) has written a forceful rejoinder to Chomsky and others who have joined him in criticising empiricist psychology and other rebuttals have come from MacCorquedale (1970) and Broadbent (1970).

MacCorquedale has protested (1970), as Skinner originally claimed, that Skinner's terms refer only to observables yet, Chomsky wins this particular round of the argument. Specific details, which would define beforehand the controlling stimuli operative on a given occasion, are not set out. Skinner's observability is a behaviouristic bogus cheque which may feature satisfactorily in the theoretical balance sheets as long as no attempt is made to cash it at

the empirical counter. If that happens, it becomes obvious that, except for the very simplest of utterances, the controlling stimuli are unknown and for practical purposes unknowable.

One of the points made by Chomsky, and most who have taken a similar position, has been that Skinner, and other S-R learning theorists who have applied their constructs to speech, fails to account for how a child learns grammar. It is very important to establish, in this connection, what is meant by the statement that a child 'knows' the grammar of a language, because this must have implications for what we mean when we say that he has 'learned' grammar. There is no dispute that a person who speaks a language, by so doing demonstrates and abides by the rules of that language's grammar - this is a matter of definition. It is usual to state that the speaker 'knows' the grammar, as a form of shorthand for this. But philosophers have long distinguished between 'knowing how' and 'knowing that' (White, 1967) and 'knowing' a grammar is surely an instance of 'knowing how', since a person who 'knows' the grammar of English in the sense used by Chomsky is only someone who can utter grammatical English sentences and need not be able to explain the grammatical rules involved any more than a person who 'knows' how to ride a bicycle must be able to give an account of the laws of dynamics and equilibrium he obeys by riding. Chomsky's own theory includes grammatical rules in its explanation of how speech comes to be produced and understood, but this does not impose any obligation upon other theorists to include grammatical rules. As Wiest puts it, 'Reduced to its essential form, some of the critics are very close in effect to saying, "Your theory does not adequately cope with the facts if it doesn't take account of my theory".'

Ingram's comment, quoted earlier, about Chomsky's review of

Verbal Behavior is accepted by the present author as true, in the sense that Chomsky appears not to have appreciated what Skinner was offering and trying to achieve in his book. Although it is true that he criticised, and naively, traditional linguistic formulations, his aim appears to have been to answer rather different questions from those which linguists have posed. Instead of asking what knowledge (in any sense of the term) a person must have to be able to speak, one could say that Skinner is concerned with why the person bothers to speak, given that he can, and no amount of structural study of language alone is likely to answer that. Chomsky gives great weight to the absence of direct evidence from experiments on speech cited by Skinner, yet the latter author states at the outset that he intends to restrict himself to showing how his descriptive methods can be applied to facts (i.e. utterances) so familiar that there is no need to establish them. At the same time, much of what Chomsky wrote in 1959 is valid criticism of the S-R approaches to language and illustrates well the limitations of Skinner's explanatory capacities.

Nothing has yet been said of Chomsky's positive contribution to linguistics, which is crucial to the present state of psycholinguistic research and theory, and which has been widely cited as an example of one of Kuhn's (1962) scientific revolutions, which involve drastic changes in paradigm or framework, rather than the discovery of important new facts.

Chomsky (1957) criticised prevailing psychological approaches to syntax along lines anticipated by Lashley (1951), who had pointed out the inadequacy of theories which treated sequences of behaviour as chains of associated responses, to cope with utterances like

'Miriam picked her son and his friend up at half-past four', in which the unit 'picked up' is split up by a noun phrase. Chomsky took the argument further and elaborated on the need to posit structures underlying such sequences which might account for how elements early in sequences presuppose the planning of later sequences. The stochastic and S-R theories criticised by Chomsky envisaged possibilities for, e.g., 'up' to be dependent upon 'picked', but not vice-versa. It is not impossible to stretch S-R theory to cover such cases, and devotees have done so since Chomsky made his point on the issue, but elegance is lost in the process.

One of Chomsky's (1957) telling arguments had to do with how children acquire language. He pointed out the implication, inherent in an explanation which relies upon reinforced imitations of utterances actually heard, that children would not be able to produce novel utterances. Children, he maintained, learn grammars, and grammars are finite systems of rules which can generate infinite numbers of sentences, through conjunction and recursiveness. In fact, Chomsky's first important point could be said to be that grammar is generative, in the mathematical sense, and that a grammar of a language should generate all and only well-formed sentences of the language. He discussed and elaborated one kind of generative grammar and added a new dimension to this, to create a new class of grammars.

The type of generative grammar which Chomsky inherited from the linguistics in which he had been trained was what he termed Phrase Structure Grammar, arrived at by the method of Immediate Constituent Analysis. This form of analysis, simply stated, amounts to a powerful refinement of the old practice of parsing sentences. Chomsky formalised immediate constituent analysis into the application of an

ordered series of 'rewrite rules', which yielded the familiar 'tree diagrams', or bracketings of sentences. The example here is from Crystal (1971):

Sentence (S)	→	Noun phrase (NP) + Verb phrase (VP)
VP	→	Verb (V) + NP
NP	→	Determiner (T) + Noun (N)
T	→	the ...
N	→	boy, girl ...
V	→	saw ...

(The boy) (saw (the girl))

This example is extremely elementary and the rules listed would only generate a small number of sentences, but phrase structure grammars are capable of describing probably all and only English sentences, if required. Chomsky, however, noticed that, among other things, such grammars do not take advantage of certain what seemed to be, self-evident relationships between sentences. Taking the previous sentence as an example, phrase structure grammar would derive quite separately from it the sentences: The girl was seen by the boy and Did the boy see the girl? Chomsky suggested that 'transformational rules' could be written, for converting one type of sentence into another related type and for conjoining and embedding, to create composite sentences. With this stroke, he established the class of Transformational Generative Grammars.

Partly as a result of work by colleagues which demonstrated limitations in the 1957 theory, Chomsky revised his theory fairly radically and elaborated some points which were only half-developed in the first version, setting out his modified approach in Aspects of the Theory of Syntax (1965). In this version, he developed two major

distinctions, between linguistic Competence and linguistic Performance and between Surface and Deep sentence structure, both of which have since been foci of attention for scholars in several disciplines. It was not until Chomsky spelled out the detailed implications of his viewpoint in the Aspects that most psychologists, even working in the language field, appreciated how radical his proposals really were, which is why, although Syntactic Structures strictly could be said to have ushered in the third phase of linguistic theorising in 1957, the previous sub-section treated the second phase as continuing until about 1960.

It would be tempting to conclude that the difference between competence and performance is nothing other than the old Saussurean contrast between la langue and la parole, and the two pairs of notions are undoubtedly related. A speaker's competence is his knowledge of the rules of his language, whereas his performance is the actual speech he utters and this, because of various physiological and psychological limits, may only rarely reflect the actual competence of the speaker. Surface structure is what is described by phrase structure grammars, whereas the deep structure of a sentence is the abstract proposition or set of propositions, which, acted upon by transformational rules, eventually yields a surface structure. A speaker's competence, therefore includes the knowledge which enables him to apply transformational rules to deep structures and to surface structures, in encoding and decoding sentences. Without going into more detail than is appropriate to the present context, it is impossible to do even rough justice to the complex arguments involved here, but Chomsky considers that, though his own concern as a linguist is to refine and improve his account of competence, and competence only, those (e.g. psychologists) whose concern is with performance must

needs incorporate linguistic competence as a component in whatever model of performance is to stand. This, in a sense, amounts to no more than stating that a specification of a person's knowledge of a language is needed before we can specify how he comes to utter well- or ill-formed sentences of that language, but a behaviourist might reply that it is superfluous to build into an account of how speech, well- or ill-formed, is uttered a model restricted to well-formed sentences, as a competence model, defined in Chomsky's terms, must be; it would amount to making an explanation of something which does not obtain an essential part of an explanation of what does obtain. Thus, we have Wiest's point, quoted earlier, that some anti-behaviourists treat their own theories as though they are data which must be explained by behaviourists, if the latter are to be taken seriously. What such a stance fails to appreciate, however, is that ill-formed sentences may be (and the evidence is that they are) ill-formed in such, non-random, ways that incorporating competence into the performance model actually enhances the parsimony of the model and, secondly, that there exists another class of data which must be covered by some or other theory: the intuitive, but nonetheless observable, judgements of speakers about the grammaticality of well- and ill-formed sentences. These latter judgements are extremely consistent and (not surprisingly, since they are the ultimate criteria against which grammars are evaluated) conform to formal grammatical rules, even though the speakers themselves may rarely utter well-formed sentences. The evidence for grammatical rules is not, as was thought by some behaviouristic linguists, that speech accords with the rules, but that speakers <u>recognise</u> when their speech does not. Any parsimony sacrificed in building an account of well-formed

sentence-generation into a performance model which has also to account for ill-formed sentences may be doubly recovered if this enables speakers' judgements upon themselves to be subsumed under the same general theory as the behaviour they thereby judge.

Chomsky added two related riders to his theory in the Aspects, the first that deep structure is universal, i.e. the deep structures of sentences are identical for all languages, and the second that it is inherited as part of a child's innate linguistic competence. This notion of an innate linguistic competence has been the focus of the more controversial argument which followed Aspects.

The Chomskyian conception of discernible, although to the superficial eye hidden, underlying orders which help to explain the contents of immediate perception has wide-ranging relevance extending even into the realm of jurisprudence (Atkinson, 1973). The controversial French anthropologist, Claude Levi-Strauss (1963), has pursued a similar conception for some years in his 'Structural Anthropology':

> *In effect, pursuing myths from tropical America to the northernmost regions of North America, I found myself confronted with the apparently paradoxical situations that the same myth manifested itself in these far flung corners of the New World. But at the same time, from one region to the other, an interior transformation evolved deep within them.*
>
> *If, for the Indians of tropical America, the passage from nature to culture is symbolised by the passage from the raw to the cooked, it is symbolised for the North American Indians by the invention of personal adornment, ornaments and clothing, and beyond that, by the introduction of trade.*
>
> *(You find, then, the same antithesis between the naked and the clothed as between the raw and the cooked?)*
>
> *Yes; that is, a mythical hero who in tropical America finds himself reduced to a state of 'crudity' before*

giving in to culture, would in North America find himself reduced to nudity.

(Levi-Strauss and Bellour, 1971)

The present author is unaware of any evidence that deep structure relationships and the occurrence of transformational rules are not universal, as Chomsky claims, and if they are universal it is hard to believe that this has no genetic basis, but it remains to be seen how direct the genetic influence is. Some authors, e.g. McNeill (1966, a & b) have taken a hard line and maintained that the 'Language Acquisition Device', with which the infant is innately equipped, is a specific set of predispositions to test particular hypotheses about the nature of language when confronted with the performance of adult speakers. Others, e.g., Slobin (1971) and Herriot (1970), have been disinclined to rule out the possibility that more general innate cognitive dispositions are sufficient. Fraser (1966), commenting upon McNeill's position, stated: 'Metaphorically speaking, a child is now born with a copy of Aspects of the Theory of Syntax tucked away somewhere inside' (p. 116). Other, more extensively reasoned, criticism of the McNeill viewpoint has been set out by Ingram (1968) and there has been at least one lampoon on the theme, by W.P. Brown (1965), who subjected to the same type of analysis as has been applied to children's speech the poetry of William McGonigall, 'The bad poet of Dundee'.

The generative grammarians' point, that a child is born able to discover the correct grammar of his native tongue when exposed to an impoverished sample of speech, and without formal instruction in its rules, has perhaps had a restricting influence upon research in this field, although it has also had a concentrating effect. It has

more recently been established th at the speech of adults to children is not the same as that of adult-to-adult (Ervin-Tripp, 1971), and it is possible that this could, when more is known on the subject, make less compelling the transformationalists' argument that the child accomplishes a task of unparalleled complexity in discovering an adequate grammar when it hears natural speech.

Transformational grammar, by stressing universal aspects of language, may perhaps have corrected an unhealthy emphasis upon differences between languages and associated patterns of thought and, by offering provocative hypotheses about speakers' knowledge and its role in speaking, may have countered behaviouristic psychology's view of grammar as a mere epiphenomenon, but there was a tendency in the nineteen-sixties to assume that all the important aspects of language could be subsumed under the study of syntax. It is impossible not to suspect that Chomsky's (1965) formulation implies that: 'Put more crudely, inaccurately and in behavioural terms, we . . . decide to utter a passive negative question and then search for some meaning that could be expressed in this format' (Robinson, 1972, p.27). Excluded from the terms of reference Chomsky has permitted himself are questions which his predecessors in linguistics, Osgood, Skinner and most social scientists would consider valid and important: questions about the functions to which language is put and how the situational context affects the way in which an utterance is framed. Our choice of 'John picked it up', instead of 'John picked up the ball', is not decided by linguistic contingencies. Johnson-Laird (1968) has identified situational determination of choice of the passive transformation. Osgood concludes a review of some similar results: 'This would suggest that the job of syntax is not central but rather

peripheral in ordinary language -- merely accommodating lexical decisions made on the basis of the fleeting interests and motivations entertained by speakers -- a notion that will surely raise the hackles of many linguists.' (1969b, p.51).

What the psycholinguistic speculations and researches of the past decade have achieved, apart from uncovering new facts and generating some useful concepts, is clarification of the issues being studied. The hypotheses now under test possess a human significance which the piecemeal ones of the ninetenn-fifties did not and Chomsky has advanced a theory which is truly psycholinguistic and not the mere borrowing by one discipline of a handful of concepts from another. If it has appeared at times, from the exclusive emphasis on competence without regard to actual practice, that linguistic rules were gaining the status of moral laws, whereas previously they had been seen, equally erroneously, as empirical laws, this may have served to show that they are different from either. A 'post-transformational age' may have arrived, in the same sense that optimistic figures refer to a 'post-Christian age' having done so: much of transformational grammar will, if this be so, figure in the approaches to be used in tackling new psycholinguistic problems and old psycholinguistic problems which were neglected during the nineteen-sixties.

1.2 INTERPERSONAL BEHAVIOUR

Psycholinguistics has advanced, since Syntactic Structures, under the stimulus of a strong theoretical challenge which has provoked important data-gathering. No equivalent clash of scientific paradigms has marked the progress of social interaction studies during this

period, although it is possible (Harre and Secord, 1972) that such a one is about to start, during the nineteen-seventies. The trend in recent years has been for studies of interpersonal behaviour to exemplify what Kuhn (1962) termed 'normal science', with broad consensus about paradigms and steady accumulation of knowledge through research within those accepted. The plural has been used here because the consensus has indulged simultaneous employment of diverse paradigms, more than the exclusive acceptance of one alone, although it is conceivable that an emergent higher-order paradigm might be sought in the consensus. Eclectic empiricism has characterised both method and theory, which have drawn freely upon techniques and concepts from engineering, biology, anthropology, linguistics and psychology.

A selection of work in the field of social interaction will be described in this section, illustrative of aspects relevant to the main concern of the author's own research.

1.2.1 Proximity between persons

In the nineteen-sixties, interest arose in the psychological importance of how far apart people stand. This was partly a side-effect of popular books, by Ardrey (1961, 1967) Morris (1967, 1969) and others, concerned with the relevance of animal to human behaviour. Much of the content of these books was anecdotal or swaggering extrapolations which are not scientifically justifiable, but the territorial spacing of animals provided a stimulus for studies of how humans manipulate space and the fact that scarcely any of the human studies have involved anything genuinely corresponding to animals'

territories, whilst it limits the degree to which the animal and human studies are theoretically relatable, does not detract from the value of knowledge about homo sapiens' spacing behaviour.

It has been found that for many animal species approaches at relatively fixed distances have characteristic and specific effects -- flight, attack, etc.-- and that members of the species tend to remain at surprisingly invariant proximities to each other. This is often conceptualised as though invisible concentric circles surrounded each animal, each circle being labelled with its behavioural significance. The most ambitious attempt to offer such a set of boundaries for the human species is that of E.T. Hall (1966), who proposed a classification into Intimate, Personal, Social-Consultive and Public Distances, each category being subdivided into Near and Far phases. Hall's system relates distance categories to the types of information perceived by the senses at the different distances, but systematic and true social validation has never been carried out, probably because of confusion over what sort of evidence would be appropriate. Since Hall implicitly borrowed a root from linguistics, in christening his field of study Proxemics (cf. Pike, 1966, on emic analysis), it might prove fruitful to seek a method for testing his classification's validity in the area of linguistics, e.g. 'variation within a frame'. One might take an informant from the culture being studied and ask him to indicate at which points, as one moved about the space surrounding him, the ongoing interpersonal relationship changed. It is highly probable, although Hall does not clearly state so, that his own methods approximated closely to such a procedure, but with the differences that the informant was unaware of being studied and that the researcher himself interpreted the informant's spontaneous reactions of intrusion, etc.,

instead of relying upon his verbal reports.

Much of E.T. Hall's writing is to do with differences between cultures in their use of space and the discomfort, all the more intense because its source is usually unidentified by the perceiver, which can result from this. Actual physical contact, a limiting case of proximity, has often been cited, by Hall and others, as an exemplar of wide cultural variation in proximity norms. The author recalls frequent embarrassment, when led about by the Arab head of department at a previous appointment, hand in hand.

Experimental social psychologists have studied proximity, usually in combination with orientation, and it has not proved difficult to show (Cook, 1970; Patterson, 1968; Sommer, 1967) that subjects report, and often adopt, different spatial relationships, according to the kind of encounter they are engaged upon. Although some of this work has been done with questionnaires, it has been well backed up by field observations. A paper by Porter et. al. (1970) has challenged the belief that proximity itself has any signalling function, with an experiment in which schoolboys, aged fifteen to seventeen years, interviewed confederates for imagined managerial positions, at varied interpersonal distances, and judged the confederates on a set of seven-point rating scales. No consistent associations between distances and ratings were found, but the experiment may be criticised on the ground that the task was unnatural from the subjects' viewpoint, since the confederates appear to have been noticeably older than themselves and highly educated (some were Ph.D.s), both of which would have been hard to conceal and likely to have a strong influence upon ratings, perhaps enough to mask any effect of distance. Furthermore, there remains a distinct possibility that the experimenters

were measuring the wrong dependent variables, with their selection of rating scales. The specific signalling role if any, of distance remains unknown at the present time, although, as will be indicated in the next but one subsection, it has a place in one serious general theory of social interaction, at least, namely that of Michael Argyle.

1.2.2 Gaze direction

Eye-contact and related phenomena have received a good deal of attention from researchers and with results which have justified the interest. Again, the influence of animal behaviour specialists has been felt. McBride (1964), at the University of Queensland, noted that poultry kept in crowded pens tend to position themselves along the boundary gazing outwards, as though to avoid meeting the gaze of their fellows. He also noted evidence of staring orders, similar to pecking orders; more dominant birds stared at their subordinates, who then looked elsewhere, apparently communicating submission. Similar ocular behaviour in primate dominance relationships has been noted by Chance (1962) and it appeared as if the same applied in humans when Strongman and Champness (1968) found that paired comparisons of a sample of subjects yielded a consistent hierarchy in resistance to looking away first when confronted with the other person's gaze. Further scrutiny (Strongman, 1970) revealed that hierarchies thus generated correlated negatively with a dominance measure from Cattel's 16PF questionnaire.

Strongman's second result would be puzzling were it not for the finding of Kendon (1967) that gaze direction plays a role in apportionment of the conversational floor. Intensive analysis of

dyadic conversations filmed by Kendon revealed a consistent tendency for a speaker to look away from his listener when assuming the floor and to glance at him only intermittently at the ends of phrases or sentences, apparently handing over the floor with a sustained gaze at the very end (cf. Figure 1.1). The listener, meanwhile, watched

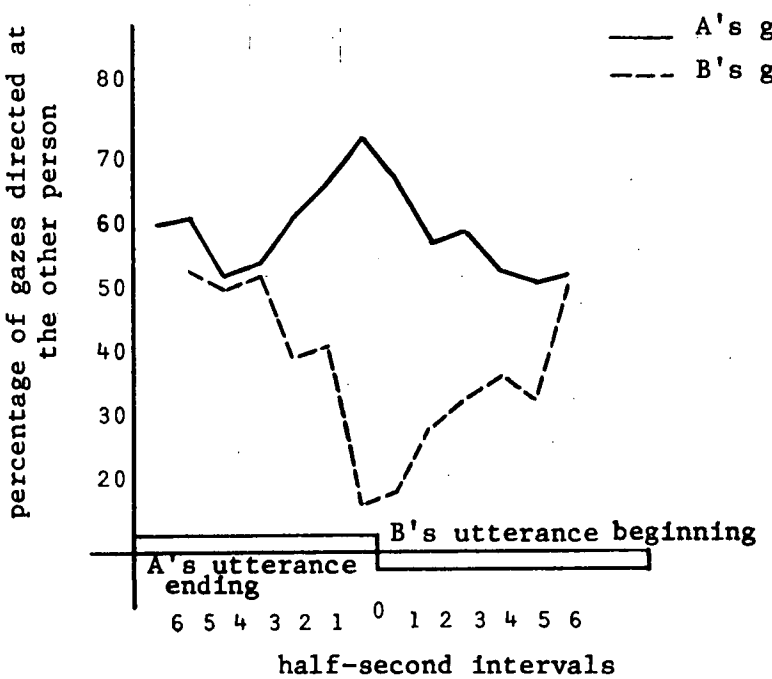

Figure 1.1 Direction of gaze at floor change points. (after Kendon, 1967)

the speaker more or less continuously during the speech and looked away in his turn when he assumed the floor. Although guilty of misusing the Chisquare statistic (Robinson, 1972 p. 138), Kendon was fortunate in the clear-cut nature of his results, which would almost certainly be unspoilt by legitimate analysis. Assuming that dominant persons are more likely to speak first than non-dominant persons, Strongman's observation that people more dominant on the 16PF test looked away more quickly than less dominant people is explicable

in terms of their having looked away because they were about to initiate conversation.

Kendon's work on gaze direction as a promoter of floor control is of especial interest since the work to be reported later is concerned with floor apportionment. Gazing away by a speaker would enable him to avoid distraction by anything the listener might say or do and gazing at the speaker would enable the listener to make best use of any visual cues the speaker might consciously or unconsciously emit. Given the existence of such a set of gaze conventions, it would of course be in a speaker's interest not to gaze at his listener as a means of avoiding interruption and it would similarly be in a listener's interest to maintain fairly steady gaze at the speaker or he might be thought inattentive (the obvious exception is when a listener wishes to gain the floor and stops looking at the speaker in order to signal this). Kendon observed particularly low likelihood for the speaker to look at the listener when speech was hesitant and therefore most under risk of interruption.

A later paper by Kendon and Cook (1969) considered the intercorrelations between different measures of gaze direction (duration of gaze, frequency of glance, etc.) and the consistency of individual differences across varied situations. The measures intercorrelated significantly and individual differences were relatively stable, turning out to be related to n-Affiliation, Dominance and Field Independence. Other research by Exline et al. (1961, 1965), Mobbs (1968) and Rutter and Stephenson (1972) has also concerned various kinds of individual and sex differences. The most consistent findings have been that women 'Look' more than men, extraverts Look more than introverts and non-neurotics more than

neurotics, although the evidence strongly indicates complicated interactive patterns involving numerous determinants of Looking.

Some interesting results have been obtained by Libby (1970, 1971) on the significance of the direction in which a person looks away from someone else, but Libby noted that his results might be partly an artefact of the spatial layout of his laboratory, so further confirmation is needed. What he has found suggests that whether one looks-away up or down is governed by cognitive factors and whether one looks-away to left or right by affective ones, with stable individual differences in readiness to maintain eye contact, correlated with age, sex and, socio-economic status.

1.2.3 The Oxford theory of eye-contact, distance and affiliation

A study by Argyle and Dean (1965) brought eye-contact and distance together. In what purported to be a perceptual experiment, subjects were instructed to approach a series of objects,'...as closely as necessary in order to be able to see the object clearly'. The objects included two photographs of Mr. Argyle, one with eyes open; the other with eyes closed. Subjects were found to approach closer to the latter than the former and Argyle hypothesised that a balance is struck between proximity and eye-contact in any encounter, such that an optimum product of the two is aimed at by participants: any increase in proximity is offset by a decrease in eye-contact, and vice-versa. The hypothesis has been developed further by Argyle (1967, 1969, Argyle and Kendon, 1967), into a general model of social interaction, based upon analogy to a motor skill (cf. Crossman, 1964).

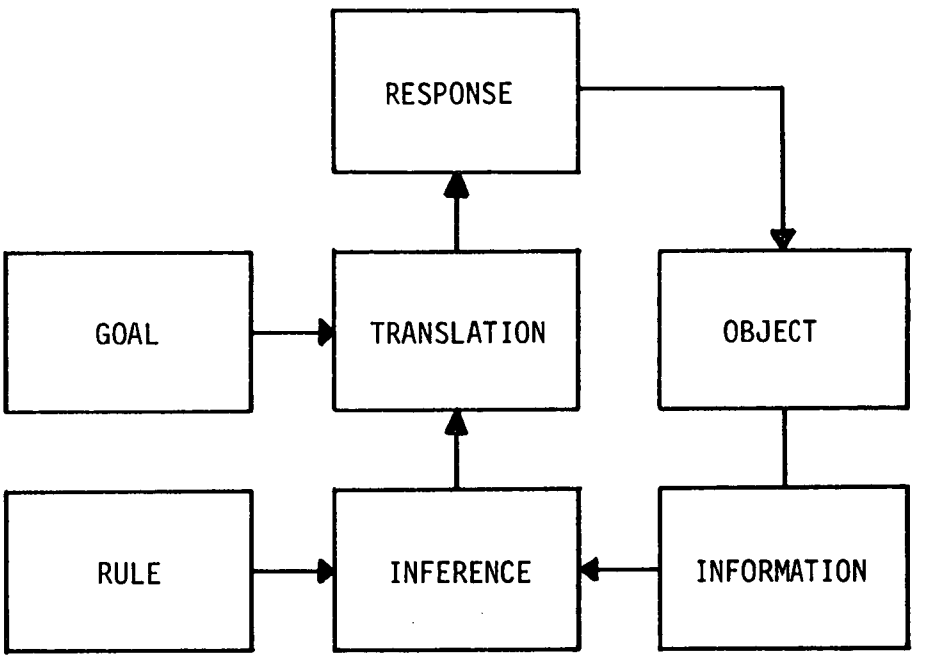

Figure 1.2 Argyle's motor skill model of human social interaction (after Cook, 1971a)

Argyle's motor skill model represents human social interaction as a complex skill, like flying an aeroplane, the major difference being that in social encounters the 'aeroplane' is also engaged in 'flying' us. Figure 1.2 illustrates the components in simple form. A person manipulates encounters so as to bring about states which correspond to his goals and monitors feedback from the other person and the surrounding environment, thence modifying his subsequent responses according to the effects of earlier ones. It has been suggested by Argyle that an important dimension of social relationships is Intimacy and that both physical propinquity and eye-contact among other variables affect the degree of intimacy in an encounter. The 'optimum product' of proximity and eye-contact previously mentioned may be replaced by an optimum level of intimacy, sought by partners to an encounter (perhaps a level struck as a

compromise between differing optima). Slack or tautness in the intimacy level, created by changes in eye-contact, distance or other factors, may be taken up by adjustments to one or more of these. It is argued that an analysis of this type can fruitfully be applied to a variety of aspects of social interaction, besides intimacy and its determinants.

The research of the Oxford University Social Skills Research Unit of recent years has been conducted within the framework of Argyle's motor skill model and there is no doubt that the model has proved extremely useful in this regard. Some researchers have observed behaviour which does not fit specific predictions from the model (Jourard and Friedman, 1970; Stephenson and Rutter, 1970; Stephenson et al., 1973), but their findings scarcely invalidate the model as a whole, since, e.g., that eye-contact and proximity both load positively upon a broad dimension of intimacy is not a necessary characteristic of the model and such posited relationships may conveniently be changed within the frame of reference it provides. In other words, in the immediate future, experiments can only throw light upon qualities of the components Argyle postulates and fill in more unknowns. Insofar as consistencies become clear, over a period of research, between the characteristics of components indicated by different sets of data, the model will have proved accurate; if no such obvious pattern emerges the model will have proved to be a red herring, but it is most unlikely that it will be disproved by one or even a series of experiments over the short term.

The strength and the limitations of the Oxford motor skill model of social behaviour both lie in its capacity to accommodate presently unforeseeable outcomes of experiments without being

invalidated. In the jargon of Personal Construct Theory (Kelly, 1955), it is highly permeable. On the one hand, it is possible to sustain investigation over a period of time without having to abandon a terminology and reference framework at each turn of the road; but on the other hand lies the danger of the model outstaying its welcome because no single crucial piece of evidence may be found to damn it and in the process, as with S-R theory, there could be some waste of time and research resources.

The main value to social psychology of the Argyle social interaction model lies probably in that it poses a large number of subsidiary research questions to which answers are likely to be readily attainable. For example, one may attempt to identify the various equilibria which people try to maintain in social interactions, the channels of communication which are employed and the significance of the different cues passed via these channels. Cook (1970) hypothesised that when our motivation towards social involvement with another person in some situation rises we will choose to increase proximity or eye-contact respectively according to whether the type of involvement is primarily affiliative or hostile in nature. His experimental results broadly confirmed this.

1.2.4 Movements of the head, body and limbs

Birdwhistell, an anthropologist, has argued that our movements form a code, susceptible to a quasi-linguistic analysis, and he coined the term Kinesics for the study of this code. There is some merit in attempting to analyse non-verbal communication as a form of code, although the applicability of the strict linguistic analogy is extremely limited on the evidence presently available. A review by

Dittman (1971) concluded that, although there were a few cases in which movements had been found to operate as discrete, categorical sources of information, in the way a true kinesic analysis would require, such instances were very rare. Furthermore, evidence that body movements are concatenated, in the way that linguistic units are concatenated to make up sentences, is meagre in the extreme. There would be a case for arguing that rarity of examples of correct kinesic syntax no more rules out the existence of such a syntax than the fact that grammatically correct sentences are rare in real life casts doubt upon the reality of grammar. However, what is missing in the kinesic realm is not so much evidence for adherence to the rules as evidence of rules to adhere to. Although most persons frequently utter ungrammatical sentences, grammar is not in the least threatened by this, because the speakers can recognise their ungrammaticality. Such evidence is totally lacking in kinesics.

Despite the shortcomings of Birdwhistell's programme for social interaction research, his actual research (1970) has received much acclaim and has inspired promising studies by others. Kendon (1970 a & b) analysed part of a film Birdwhistell had made of social interaction in a London pub and reported that a speaker's body movements were closely coordinated with his speech. Listeners' movements were also coordinated with the speech, but it appeared likely that this was indirect, resulting, at least partly, from their being coordinated with the speaker's body movements. The method Kendon used was to 'parse', independently, transcripts of the speech and the movements of all persons filmed. The verbal and non-verbal structures thus obtained were superimposed and close approximations to isomorphism were observed, from gross body movements accompanying

major breaks in dialogue to eyebrow and finger movements marking phrases and syllabic rhythms. Unfortunately, this approach lends itself poorly to statistical estimates of actual degree of correspondence between speech and movement or the probability of observing a given level of isomorphism on a chance basis, so that a psychologist is more than a little at loss for criteria by which to judge the significance of what is reported. As yet, Kendon has published analyses of only a tiny fragment of film, which may or may not be typical in speech-movement coordination and may have been, deliberately or not, pre-selected. Moreover, Kendon specifically excluded from analysis movements which were obviously instrumental in nature.

There is an interesting and potentially important issue raised by Kendon's research, one which overhangs the whole kinesic-linguistic analogy: should the explanation of such regularities as he has observed be kept separate from that of the numerous irregularities which must obtain, or should a single theory which accounts for both be sought? The Chomskyian school of linguists has argued, apparently to the conviction of many contemporary psychologists of language, that it is both justifiable and necessary to create a theory of linguistic competence with scant applicability to actual verbal performance, accounting only for those utterances which native speakers judge grammatical. In the kinesic case the situation is not quite analogous, however, for nothing has been shown to correspond to the native speaker's intuitions about grammaticality, nor which would form an acceptable basis for excluding from analysis movements uncoordinated with speech.

1.2.5 Floor apportionment

The motor skill model portrays a social encounter as the transmission of information to and fro, by several channels broadly distinguishable into the two categories verbal and non-verbal. Actual verbal content normally forms an ostensible primary stream of messages and the paralinguistic and non-verbal accompaniment functions to back up and to add nuances to what is said. But, as must be clear from what has proceeded, non-verbal communication also plays a role in regulating the encounter so as to facilitate what goes on via the verbal channel. Non-verbal signals of assent or disagreement and of incomprehension provide feedback to a speaker and help him to adjust his verbal messages, without actually interrupting him. A great deal of non-verbal communication probably has to do with the timing of participants' remarks, which needs to be achieved efficiently if each person is to get a fair chance to speak without either unwanted silences or interruptions. Non-verbal adumbration of intent to speak or stop speaking is a feature of small group conversations probably no less than of large committee meetings.

The extent to which control of the conversational floor is necessary by some means or other can be illustrated by imagining two robots, each of which is programmed to speak for fifty per cent of the time according to a random schedule, holding a conversation. During twenty-five per cent of the total time, neither robot would be speaking and during another twenty-five per cent of the time both would be speaking at once. To some extent it is true that the actual content of what one person says facilitates apportionment of the floor, but Argyle (1972, p.12) claims that linguists, at least, have in the past

underestimated the extent to which non-verbal cues are relied upon, although Robinson (1972, p. 138) suggests that Argyle is premature in this conclusion, proposing experiments in which linguistic and non-linguistic floor control cues are compared for effectiveness. What Robinson slightly neglects is that the use actually made of non-verbal cues may be much greater than the actual necessity for them to be used, since people may well operate with what is nominally high redundancy of cues in real life encounters. The kind of experiment he proposes would be likely to reveal more about how well speakers can manage under limiting conditions without verbal or non-verbal cues than how much use we normally make of them, unless carried out under very natural field conditions.

Considering what kinds of verbal and non-verbal cues are available for use in floor-regulation, the most obvious ones are meaning and syntactical markers. Utterances are rarely meant to end with 'the' but interrogative sentence forms normally do imply a handing over of the floor. However, everyday speech is sufficiently irregular for non-linguistic cues to be less ambiguous and this is another point which Robinson fails to appreciate. Maclay and Osgood provide an example, from which the following is taken, of how strictly irregular even educated speech often is:

> *As far as I know, no one yet has done the - in a way obvious now and interesting problem of doing a - in a sense a structural frequency study of the alternative syntactical - um in a given language, say, like English, the alternative um possible structures, and how - what their hierarchal probability of occurrence structure is. Now, it seems to me you w-w-will need that kind of data as a base line from which to judge or study deviations -in particular style in the clinical situation and so on. If we get this - now in other words, if you find that in the clinic, say in the um protocol of a patient, that the distribution*

> *of these um alternative structures are precisely what they are in ordinary um communications, then there's no evidence that this, at least i-is a relevant variable for the clinical situation. (etc., etc.)*
>
> (1959, p. 25)

Not only is the grammar in the above passage confused, but the meaning of the words, too, is obscure, at least until given careful scrutiny. Yet no one would doubt that what was said may have been quite easy to follow when it was originally uttered. Floor-regulation, without benefit of non-verbal cues would not be very easy if the verbal sequences themselves were similar to this one.

Schegloff (1968) studied telephone conversations in depth and claimed that the verbal content follows sequences of summonses and answers. Although certain non-linguistic cues remain in the telephone medium, it is noteworthy that floor-control should become so prominent in the verbal exchange itself when visual information is lost. Robinson (1972, p. 142) noted how explicit radio-telephone rules are, yet oddly did not see their relevance, as accommodations to eliminations of visual cues, to Argyle's claims about non-verbal floor regulation. Studies of conversations between blind persons might prove fruitful to this area of research.

Intonation is undoubtedly a useful source of floor-regulatory information and probably plays an especially important role in telephone conversations. Linguistics has possessed conventions for the transcription of intonation for a long time and these have occasionally been used in the analysis of verbal records by psychologists (e.g. Boomer, 1965; Dittman and Llewellyn, 1969). The disrupted syntax typical of spontaneous speech would presumably affect its value

in floor control, although whether to increase it or decrease it is another question.

Kendon's (1967) study of the role of the eyes in encounter regulation has previously been described. The relevance of his more recent (1970, a & b) studies of body movement to control of the floor is something which will presumably emerge with future publications, but it is obvious that if movements are conventionally coordinated with speech it should be possible for speakers and listeners to foreshadow kinesically their intent to speak or be silent. Dittman and Llewellyn (1968) found head nods were used both, when accompanied by vocalisations like 'uh-huh', to signify assent and confirm that the other person still held the floor and, when done vigorously and with appropriate sounds, in order to claim the floor. There is evidently ample room for more research in this area, but the case seems clear that, whether necessary or not in any absolute sense, non-verbal cues do perform floor-apportionment functions of various sorts.

The experiments to be reported in Chapters Three and Four of this thesis are concerned mainly with floor control by non-verbal means, but non-verbal means of a vocal nature. The fact that visual cues are used in floor control provides one basis for testing hypotheses about vocal ones, since it is easy to block the visual channel and to observe any compensatory increase in the vocal cues. Such a research tactic, of course, follows logically from the Argyle motor skill model of social interaction, which is about very much this sort of inter-channel compensatory process.

1.3 SOCIOLINGUISTICS

If Osgood (cf. quotation in penultimate paragraph of 1.1.2) was at all correct in arguing that syntax merely accommodates lexical decisions which reflect 'the fleeting interests and motivations' of speakers, a good deal of the explanation of speech rests in how those interests and motivations come to pass and how they influence choice of syntax. It is likely that much of this has to do with social relationships and momentary circumstances, which places it squarely in the sociolinguistic arena. To many of the present-day casual literati, the term 'sociolinguistics' implies something to do with the interpretation of working class school failure put forward by Dr. Basil Bernstein (1958-1972); to the older-fashioned it suggests the writings on language, thought and culture of Edward Sapir (1921) and Benjamin Lee Whorf (1956). Both examples are legitimate concerns of sociolinguistics, but they make rather unrepresentative samples of the discipline, which encompasses a great variety of studies, each involving in one or another way speech or writing in relation to social phenomena. In the present section, some of the significant material from these studies will be identified and considered, before moving on, in Chapter Two, to research on hesitations in speech.

1.3.1 Taxonomical efforts in sociolinguistics

Perhaps as a consequence of the pre-paradigmatic state of sociolinguistics, there has been some pre-occupation with taxonomy. Hymes (1967, 1972) suggested an amusing mnemonic to assist classifying the components of speech events, based upon the word SPEAKING: Setting, Participants, Ends, Art characteristics, Key,

Instrumentalities, Norms (of interaction and interpretation) and Genres. Unfortunately, some of Hyme's terms need more elucidation than the mnemonic economy justifies, in order to be understood, and the value of the taxonomy is limited by the obvious all-inclusiveness of such aspects as Key and Genre.

Susan Ervin-Tripp (1969) has attempted a preliminary classification of sociolinguistic rules into the categories of Alternation, Co-occurrence and Sequence Rules. Whereas the second and third terms, meaning respectively rules determining what verbal elements may feature in the same utterance or dialogue and how they may be ordered, fit their referents well, the first is a misnomer ('...a disastrous lexical choice...', Robinson, 1972) because of implied temporal cyclicity. Robinson preferred the term Rules of Substitution, although even that is a limited improvement, to cover rules which specify the range of choices available to a speaker at all levels of linguistic analysis. Thus, Substitution rules would concern choice of a formal or 'matey-matey' (Kaldor, 1973) style of speech on the telephone; Co-occurrence rules would determine whether both formal and 'matey-matey' may be used in the same utterance and whether formal syntax may be spoken in conjunction with 'matey-matey' phonology; Rules of Sequence would influence whether 'matey-matey' should follow or be followed by formal speech.

Ervin-Tripp's sociolinguistic rules offer more promise than quite a few proposals in the field, if only because she has withstood the temptation to let the number of types multiply beyond what is well-founded in the present state of knowledge (unlike Hymes, who, with SPEAKING, committed himself to eight classes). There is still the issue of whether her choice of categories will endure over the long

term and this is hard to predict because there is no knowing what sort of evidence would be relevant to its retention or abandonment. The very notion of sociolinguistic rules at all and the examples provided are the most important contributions of Ervin-Tripp's paper, raising numerous questions to do with their nature. The contemporary acceptability of rules as constructs in social scientific theorising is a legacy of the Chomskyian 'revolution' proper and there is strong temptation to expect sociolinguistic rules to have qualities similar to rules of generative syntax, though whether they do in reality is, as Kaldor (1973) pointed out, a question that remains to be answered. One prediction the present writer would make, concerning rules of the sort Ervin-Tripp has formulated, is that they will be found to be over-simple, in the sense that we probably operate with rules that combine features of all three. To concoct an example: in situations where acquaintanceships are being formed, formal styles of speech must precede informal styles, which may be introduced at the level of phonology before that of syntax.

In a very recent paper, presented at the same symposium as Kaldor's, Taylor (1973) presented 'A schema for the contextual study of language', in which he divided the Linguistic and Non-linguistic domains of context each into Antecedent, Concomitant and Extrinsic features, with examples of each (Figure 1.3). Studied criticism of the taxonomy will have to await availability in printed form of the full paper, but the immediately striking oddity about it is the contrast of Extrinsic with Antecedent and Concomitant features. A normal taxonomic working rule is to attempt mutual exclusiveness as well as exhaustiveness of classes but, although these ideals are neither seldom realised, Taylor's does not even approach the first in the respect mentioned.

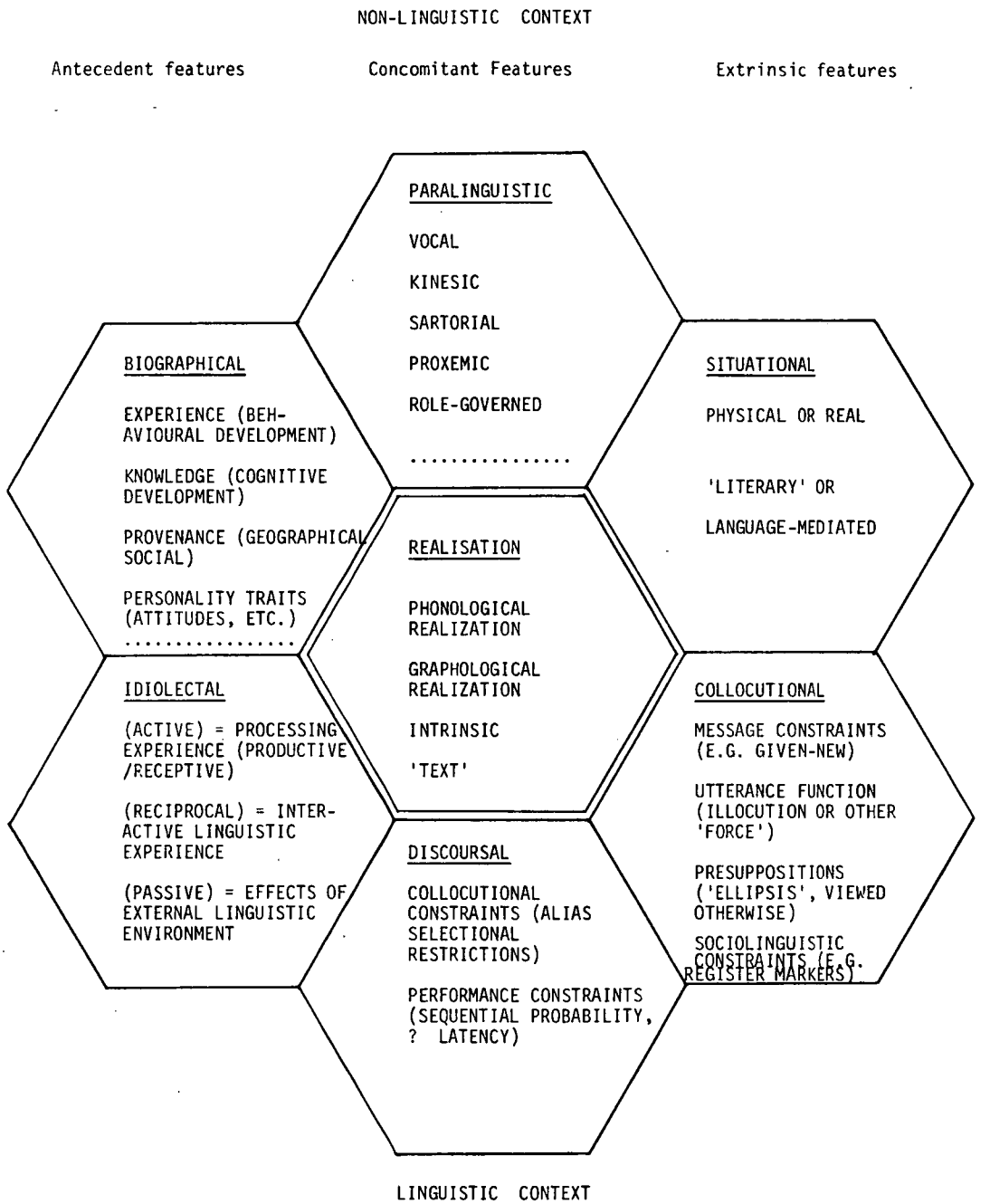

Figure 1.3 A classification system for sociolinguistic contexts (after Taylor, 1973)

The most ambitious and carefully thought out sociolinguistic taxonomy of which the author is aware is that of Robinson (1972), who distinguishes fourteen different functions for language and lists the everyday names for and primary foci of the verbal acts, with their linguistic forms, bases of evaluation as successful or not and one example or more of each (Table 1.1). Robinson is explicit about what should be required of a taxonomy for language functions, mentioning exhaustiveness, clear verbal and operational definition, the inclusion of certain relevant but strictly non-verbal aspects of talking and the realisation that any taxonomy achieved will in practice prove inadequate. Robinson provides support for his taxonomy by demonstrating its usefulness as a framework for the contents of his book. Ultimately, such things as taxonomies have to be judged in terms of their usefulness, rather than experimental results, although successful taxonomies tend to acquire status as theories in their own right, and Robinson's shows promise, both as a layout for a book on sociolinguistics and in pointing to directions for systematic research, if only by occasional gaps in the table. The taxonomy itself represents an elaboration of one tentatively put forward by Jakobson (1960), within functions added and with more fine differentiation. If Robinson's improvement proves a success, this will presumably be seen through still further differentiation of the system, in the light of research within its frame of reference.

TABLE 1.1

FUNCTIONS OF LANGUAGE (after Robinson, 1972 pp. 51-52)

FUNCTION	EVERYDAY NAME OF ACTIVITY OR PRODUCTS	PRIME FOCUS OF VERBAL ACT	PRIMITIVE LINGUISTIC FORMS - GENERAL	EXAMPLES	BASIS OF EVALUATION
1. AVOIDANCE WORSE ACTIVITY	ESCAPISM(VERBAL)	PARTICIPANTS	ANY WITHIN CONSTRAINTS OF CONTEXT		SAVED FROM OTHER PROBLEMS?
2. CONFORMITY TO NORMS	SPEECH AND WRITING	RULE SUBSCRIPTION	ANY WITHIN CONSTRAINTS OF CONTEXT		DID YOU KEEP GOING WITHOUT AWKWARD SILENCES?
3. AESTHETICS	LITERATURE, POETRY, DRAMA, NOVEL, RHETORIC	MESSAGE FORM	OFTEN WELL DEFINED, BUT ABOVE RANK OF SENTENCE. BEYOND MICRO-LINGUISTICS	SONNETS; TRAGEDIES; FABLES	BEAUTIFUL, INSIGHTFUL, MOVING?
4. ENCOUNTER REGULATION	GREETING, LEAVE TAKING	PARTICIPANT INTERACTION	A FINITE SET OF SPECIAL WORDS, NOISES, AND PHRASES. PAUSING, QUESTIONS	HI! JANE! CHOW! WHAT DO YOU THINK?	ATTENTION ATTRACTED? CONTACT MADE? FLOW MAINTAINED? ENDING SATISFACTORY?
5. PERFORMATIVES	PROMISING, BETTING, ETC.	NON-VERBAL ACCOMPLISHMENTS	A FINITE SET OF SEMANTICALLY ASSOCIATED VERBS USED IN NORMATIVELY AND LEGALLY PRESCRIBED FORMS	I NAME THIS SHIP THE Bubbly Bosun	INTENDED ACT PERFORMED?
6. REGULATION OF SELF i) behaviour ii) affect	TALKING TO ONESELF, PRAYER, ETC.	EMITTER	ABBREVIATED IMPERATIVES?	NOW, ONE TEASPOON MUSTARD, PULL YOURSELF TOGETHER	IS PERFORMANCE IN FACT FACILITATED BY TALKING? IS AFFECTIVE STATE CHANGED OR INDUCED?
7. REGULATION OF OTHERS i) behaviour ii) affect	COMMANDS, REQUESTS, THREATS, JIBES, JOKES	RECEIVER	IMPERATIVES, CLOSED QUESTIONS, MODEL VERBS, ETC. A FINITE SET OF SEMANTICALLY ASSOCIATED VERBS AND PHRASES. SET FORMS OF HUMOUR	JUMP! WILL YOU...? YOU MUST ... IF ... THEN ... YOU CREEP. PUNS; SICK JOKES	OBEDIENCE OBTAINED? DISSUADED? HUMILIATED? MADE TO LAUGH?
8. EXPRESSION OF AFFECT	EXCLAMATIONS, SWEARING	EMITTER	VOCATIVES, SWEAR WORDS, TERMS OF ENDEARMENT	OH MY LOVE!xxxx!	DO YOU FEEL BETTER?
9. MARKING OF EMITTER i)emotional state ii)personality iii)identity	- - -		PARA- AND EXTRA-LINGUISTIC FEATURES; OVERT STATEMENTS. PHONOLOGY (ACCENT) GRAMMATICAL,LEXICAL CHOICES	I, I, I THINK... I'M SCARED. 'OTEL; AIN'T NO... LAVATORY	CORRECT DIAGNOSIS MADE OR IMPRESSION CONVEYED?
10. ROLE RELATIONSHIP MARKING	-	RELATIONSHIP EMITTER RECEIVER	RIGHTS AND DUTIES TO USE OF SOCIALLY PRESCRIBED FORMS OF ADDRESS, AND FORMS OF UTTERANCE	SIR! SWEETIE! LET US PRAY	CHOICE AND SEQUENCE RIGHT FOR ACCEPTED WAYS OF DEFINING ROLES?
11. REFERENCE TO NON-LINGUISTIC WORLD INVOLVING: DISCRIMINATION, ORGANIZATION, STORAGE, TRANSMISSION, INSTRUCTION IN SPHERES OF KNOWLEDGE: (i) logics, (ii)science (iii) ethics, (iv)metaphysics, (v) aesthetics	MANY: STATING, ARGUING, REPORTING, REMEMBERING, THINKING(?) PROBLEM-SOLVING, ANALYSING, PROCESSING, SYNTHESIZING	CORRESPONDENCE OF VERBAL ACT TO NON-VERBAL WORLD	DECLARATIVE SENTENCE FORMS	THE CAT IS ON THE MAT. IF A, THEN B! DOGGIE WILL BITE! ALL GONE, DADDY	TRUE OR FALSE WITHIN PREMISSES OF UNIVERSE OF DISCOURSE? IS ARGUMENT VALID? ARE RULES OF GAME FOLLOWED?
12. INSTRUCTION	TEACHING	ACQUISITION NEW SKILLS BY RECEIVER	VARIOUS		DID YOU LEARN?
13. INQUIRY	QUESTIONING	ACQUISITION KNOWLEDGE FOR EMITTER	INTERROGATIVE FORM	WHAT IS HE ON ABOUT?	DOES IT SERVE TO FILL THE APPROPRIATE GAP IN YOUR KNOWLEDGE?
14. META-LANGUAGE FUNCTIONS	-	-	-	-	-

1.3.2 Language and Culture

It is not proposed to cover this heading in much detail, partly because much of what would otherwise be written here belongs in 1.3.3 and partly because there has recently been a diminution of interest in what was previously the prominent issue in the field.

It is possible to trace explicit interest in language as a medium for the transmission of culture, and thence thought, back to von Humboldt and beyond, but the mid-twentieth century saw considerable interest in the hypothesis, advanced in a radical form by Benjamin Lee Whorf (1956), who argued that a language imbues its native speakers with a cosmology or metaphysical outlook which constrains them to think about and even perceive reality in particular ways from which it is exceedingly difficult, not to say impossible, for them to escape. Whorf had learned linguistics at the feet of Sapir, who was steeped in the relationships between language and culture and who had himself been taught by the father of American empirical linguistics, Franz Boas. It is possible to trace the emergence of the linguistic relativity and determinism hypotheses in their Whorfian form through the careers of these three men.

Whorf's own evidence for his thesis was unfortunately inconclusive, although fascinating to the reader, as most of the inference was circular, linguistic evidence for exotic cognition being correlated (anecdotally, not statistically) with linguistic forms. The authoritative critique of the field as it stood at the end of the nineteen-fifties (Fishman, 1960) pointed out that the essential Sapir-Whorf hypothesis could be formulated at different levels, representing varying degrees of strength and interrelationships between

different sources of evidence. Whorf drew conclusions at the strong levels of the hypothesis, but actually evinced interrelationships relevant only to the weak forms.

Other investigators in the field have produced quite a lot of facts (reviewed by Slobin, 1971) consistent with various weak forms of the linguistic determinism hypothesis, but evidence to support the strong formulations has not been forthcoming. Interest in the issue dwindled during the nineteen-sixties, as the Zeitgeist swung towards universals of language, and Chomsky has claimed that Whorf was over-concerned with surface instead of deep linguistic structure, a peculiar criticism for which to single out Whorf since he was sixteen years in his grave before Chomsky himself began developing the notions of surface and deep structure as they are now understood. It may well prove to be a mistake to contrast the positions of Whorf and Chomsky, although the temptation is strong, on a superficial observation of their respective emphases upon differences and similarities between languages. In fact, Chomsky's definitive work has been carried out in reaction against the Bloomfieldian conception of linguistics, which some writers like to contrast with Sapir's approach, so that one might expect there to be a wide area of agreement between the contemporary Chomsky and, if he were still alive, Benjamin Lee Whorf. Such is the conclusion of Peel (1972, p. 64). Unlike Bloomfield and the early Chomsky, both the later Chomsky and Whorf have shown a great concern with meaning, as well as grammar.

1.3.3 Language and social structure

Even in the present day, human societies vary enormously in structure, from primitive hunting bands in the Kalahari, South America

and Asia, to the complex, multi-million communities of the advanced world. Indeed, the full range may be spanned by a single nation state, as in Australia and the Republic of South Africa. Without committment to any recognisable version of the Sapir-Whorf thesis, it is still possible to seek correlations between how societies are organised and their use of language.

Historically, one may point to the cases of Imperial China and Egypt as suitable for sociolinguistic speculation. Despite wide variation in actual language, China possessed a common form of writing which may have made up in its politically unifying influence what was lost in communicative efficiency between members of the same language community. According to Coon (1962a), a single Hamitic language was spoken from the Delta to the First Cataract in ancient Egypt. This may have had some unifying influence on the Nile culture, although it needs to be admitted that there were in fact two separate polities involved and that the river itself provided a unifying influence which may have brought about what linguistic homogeneity there was. Goody and Watt (1962), referring to China and Egypt, point-out that the difficulty of learning to read and write in ideographic systems fostered the growth of powerful commercial and administrative elites, which in turn helped to make the polities conservative and relatively stable (the Chinese civil service examinations scarcely changed from before the time of Christ to the overthrow of the Manchu dynasty in 1911). In more recent centuries, a powerful unifying influence upon the dislocated Arab civilisation, which in AD 715 straggled from Lisbon to Mandalay, has been the common formal language of High Arabic, alongside that of Islamic religion. Mazrui (1971) points out a mutually supportive aspect in the standardising influence

of the Q'uran, which was believed to be the very syllables of Allah.

Pervading themes in the study of both animal and human social behaviour are those of Dominance and Authority. Indeed there is a temptation to treat social structure as though it amounted to no more than how authority is apportioned, so there is reason to consider speech in relation to authority.

Most societies have special verbal forms for use when addressing or referring to authority figures: 'Your Majesty', 'The Right Honourable'. Newly independent countries have created their own, the term Mwalimu (Kiswahili, 'teacher') e.g. becoming the Tanzanian mode of address to President Julius Nyerere (who was once a schoolteacher). Some societies have actually reserved special languages for use by, between, or when addressing authority figures, like the 'honorific' languages of Polynesia (Mead, 1928), the 'Badu-badu' of the Pilbara region (von Brandenstein, 1973) and examples of elitism in Europe like Court French in Tsarist Russia and Norman England. These special variants are certainly amenable to substantial sub-classification on both social and linguistic grounds, e.g., according to the roles of those who speak them and whether the variant is substantially a different language or just a florid form of the normal tongue of the community.

In many pre-literate societies, proverbs fulfill important quasi-legal functions and their usage is governed by sometimes complicated rules. Arewa and Dundes (1964) have reported on Yoruba, in which certain proverbs may be uttered only by or between particular groups of individuals, in certain situations, or both. One of their informants was reported to say, 'I know the proverbs, but I don't know how to apply them'.

Gumperz (1964) identified not only Hindi, the language of communication between villages, but also three distinguishable variants of local dialect used within a village, in Khalapur, North-West India, where the caste system provides an exquisite social backdrop to linguistic studies. Between Rajputs and Untouchables, within family or other close circles and to servants or animals, a variant Gumperz termed Motī bolī was used, with connotations of informality; Safi boli, a more guarded form, was the norm for other encounters within the village, while a third, 'regional' variety existed for talking to pedlars and travelling entertainers. Ervin-Tripp (1969) refers to an observation in India by Ramanujan of Brahmins consciously choosing verbal forms which identify them. Nancy Mitford (1955) had written about similar behaviour on the part of the English upper classes, with the added complication that, in a socially mobile society, they continually have to revise their norms to foil interlopers.

Albert (1964) provides a lucid account of usage in Burundi, a traditional kingdom (now republic) in East Central Africa which still approximates closely to a true feudal system, a stocky Bantu people, the Bahutu, making up most of the populace with, as the upper strata, the statuesque Batutsi, descendants of a conquering Nilo-Hamitic tribe, which adopted the Bantu language of its serfs and, at the bottom of the social ladder, the pygmoid Batwa. Speech training in Burundi involves practice at impromptu speeches, defensive rhetoric and proper forms for petitioning as well as knowledge of speakers' precedence, based upon status and age. It is interesting to note that practice when policies of any kind are being discussed is for speakers to preface their remarks with a more or less standard preamble in which they express agreement with their superiors' remarks, even though they may be about to expound

diametrically opposed views. Burundi is typical of most African cultures in being basically horticultural and relatively high in food-accumulation. Such communities (cf. Barry et al., 1959) are characterised by prescribed roles, positional, rather than personal, influence and strong social conformity. The apparently ingratiating behaviour is regarded merely as proper politeness and respect. Choice of verbal tactics in Western societies is more open and the Kirundi style of public speaking would be likely to convey impressions here of confusion, excessive tact or a naive attempt at low manipulation.

Albert observed in Burundi special forms of speech for women and children and the teaching, interestingly indeed, of proper patterns for eye-contact and gesture. Of particular note is feigning by social inferiors of verbal incompetence, even though the same speakers may be perfectly fluent and facile in their turn, when addressing subordinates. Something similar is familiar to most who have watched films featuring American Negro servants.

In a Western cultural setting, the research of Roger Brown and his Harvard colleagues into modes of address (Brown and Gilman, 1960; Brown and Ford, 1961) is among the best known in sociolinguistic literature. Designating the second personal pronoun singular T and the second personal pronoun plural V (from the Latin and Romance forms) they traced in history their respective usage to address a single person. Addressing a single person as V appears to have begun with the Roman Emperor, when the empire split and a second emperor sat in Constantinople, as a means of indicating a continuing nominal unity, and to have spread downward through society establishing a generalised non-reciprocal usage based on status: superiors were addressed with V; inferiors with T. This 'Status norm' prevailed throughout mediaeval

Christendom, with an emerging practice for equals to use reciprocal V if of high rank or T if commoners. Until the Reformation, at least, the linguistic usage was in harmony with the hierarchical structure of European societies, which extended beyond human adults through the saints and angels to God, in an upward direction and through children to animals downwards.

Beginning with protestantism, there has been a persistent egalitarian trend in Western Europe for some four centuries and the old status-governed use of pronouns has been increasingly at odds with the implicit cultural conceptions of Man, resulting in its being supplanted by a 'Solidarity semantic', reflecting horizontal differences between outgroup and ingroup by application of reciprocal T for intimate and reciprocal V for formal relationships. The direction of association has probably been determined by the emergence of what the Marxians would term 'class consciousness of the proletariat': the ingroup-outgroup polarity was first appreciated by the lower orders who already used reciprocal T among themselves and V to outsiders (i.e., their betters). The new norm was encouraged by political dissenters and, as it established itself, was adopted by the aristocracy, probably through sheer force of numbers, as with English in England and Rurundi in Burundi.

Cultural egalitarianism has yet to become absolute, however, even in the most radical of European countries. The Papal and Royal Plurals persist and linguistic usage reflects a contemporary state of unstable equilibrium. The Status and Solidarity norms coexist, with occasional ambivalent social situations making linguistic choice difficult and with an ever-present possibility of causing,

deliberately or not, offence by use of the 'wrong' pronoun.

Use of T ('thou') is rare in the Anglo-Saxon world, except in local dialects and religious practice, and the paper by Brown and Ford (1961) considered the use of first names (FN) and last name, preceded by a style or title, (TLN) in marking status and intimacy. Practice broadly follows that for pronouns among Continentals, with FN indicating either familiarity or that the addressee is of lower status and TLN formality or the speaker's lower status. Usage is more complex than the present account has space for, but the main difference of any interest between English and Continental forms results from the greater freedom that the English language permits, since FN and TLN do not exhaust all possibilities: address can be completely unmarked for either status or solidarity by omitting any name at all and there are also titles alone, diminutive forms of titles ('Sarge', 'Skip'), nicknames proper and multiple naming (free variation within a class). The Continental and Celtic languages are not without some of these devices altogether, being particularly rich in diminutives, but pronouns and how verbs are conjugated are inescapable markers of status or solidarity, so that the equivalents of Anglo-Saxon forms add nuances, rather than perform the main semantic function. Robinson (1972) has pointed out the lack of research into the effects of switches between modes of address, but it is probably true that there has been little systematic study of the situational determinants of switches either, in cases when a speaker varies his address to a particular person.

It is often assumed that Australia is a more egalitarian society than Great Britain and relatively more favour for FN above

TLN is easily observable in the former country. The precise significance of this is easy to misinterpret, as Argyle has done (1969, p. 82), concluding that evidence from Richardson (1961) indicated dislike by British immigrants of Australian linguistic over-familiarity. What Richardson actually found was that disgruntled immigrants cited this as objectionable, but the proportion of his whole immigrant sample who did so was almost exactly the same as for a native Australian control group. Both Richardson's data and this writer's questioning of Australian informants indicate clearly that naming is used in this country, as in Britain, to mark both solidarity and status and Australian interpretations of other persons' modes of address seem to be very similar to those of the English: salesmen in Australia call customers by FN more, but Australian customers, like their English counterparts, interpret this as a sales manoeuvre rather than any genuine indication of special solidarity. At the same time, there would appear to be grounds for thinking that the solidarity norm is relatively more predominant than in Britain.

Information on modes of address in non-IndoEuropean societies reinforces strikingly the general import of Western research. Ervin-Tripp (1969) describes results from Geohegan in the Philippines and Howell in Korea, which show relative rank as the prime criterion for address, followed by relative age, with friendship (Philippines) and solidarity (Korea) in third place. Use of the Rurundi second person singular and plural as familiar and respectful modes of address is reported by Albert (1964), who adds that switching between modes is possible within a conversation, as appropriate. This last remarkable parallel to European speech cannot be attributed to

cultural diffusion in the colonial period, which was short and split between two imperial powers, with three metropolitan languages anyway.

Coon (1962, a & b) maintains that both biological and cultural evolution repeat themselves and that parallel evolution has been underestimated by social scientists, who over-stress the role of diffusion. Burundi has a geographically contiguous twin ex-kingdom, Rwanda, identical in size, terrain, language and social structure, with which it was jointly administered under German and Belgian rule, and it is almost impossible not to believe that the two began as one and split, like neighbouring Bunyoro-Butoro in Uganda. Could it be that Rome and Byzantium have a sociolinguistic parallel in East Central Africa?

The ubiquity of status and intimacy, or features scarcely distinguishable from these, as determinants of how people address each other is almost enough to justify the term 'sociolinguistic universal', especially now that the term 'universal' in theoretical linguistics has been weakly redefined (Chomsky, 1965) and with regard to the listing of cultural universals by Murdock (1943).

1.3.4 Social class and Bernstein's theory of sociolinguistic codes

Quite a different study of language use and social structure sprang from concern in the 1950s at educational under-achievement of lower class children. Observations of intelligence test performance and speech in lower working and middle class boys led Bernstein (1958) to hypothesise that social classes are inclined towards different uses of language and that a lower working class orientation handicaps

children at school.

Bernstein (1961, 1965, 1972) distinguishes between two ideal types of speech, in terms of the social relationships between the speakers. When they share the assumption that each is a unique individual, with distinct experience, values and knowledge, a form of speech will tend to be generated which makes meaning explicit and which Bernstein christened the Elaborated Code. When, by contrast, speakers and hearers share much in common experience, values and knowledge, are aware of this and feel that they belong to some in-group, meaning will tend to be more implicit, since in context it will still be capable of being understood. Gestures may bear important meaning. Such utterances function, not to convey information so much as to adjust or reinforce in-group relationships and for it Bernstein coined the term, Restricted Code.

The definitions of restricted and elaborated codes are anchored in Bernstein's conceptions of the speaker-hearers' social milieux. However, he went on to propose that the British social classes differ socioculturally, so that the middle class tend to switch between a number of elaborated and restricted codes as changing interpersonal roles of encounters demand, whereas lower working class circumstances approach a limit at which only restricted code speech is generated. Education systems in most countries of the world (although some disciples of Bernstein almost presume only in 'Capitalist' countries), attempt to impart elaborated code speech and writing and usually presuppose a modicum of competence in the former when school begins. Hence, children whose sole experience of language is restricted code and who belong to an inclusive sub-culture in which experiences unique to individuals rarely occur may find

themselves at the age of five, confronted with school where language possesses a quite novel function which they are expected to comprehend unassisted. Indeed it could hardly be explained verbally except via an elaborated code.

Nearly all the empirical research on Bernstein's proposals has concerned social class and educability. Yet he propounded a systematic general sociolinguistic theory which impinges on role theory, psycholinguistics and several other fields of social science, and which is elaborated in some directions to a fairly considerable degree, e.g. on different types of restricted and elaborated codes. Social class is actually incidental to the theoretical work, since it is only on the basis of known or hypothesised states of affairs in the different social classes that predictions about their code use are possible at all. Such predictions do not follow from the mere existence, per se, of social classes. The full Bernsteinian theory demands if not chronologically then logically, a two-stage test, the first stage determining whether assumptions shared by speaker and hearer evoke speech in a more restricted code than their absence. Beyond the first stage, one would envisage examining social classes for both their internal social relationships and their speech. The roles of sociolinguistic codes in education would remain even at this stage, still to be studied. In the event, the theoretical cart has been put before the research horse and whatever useful knowledge has been obtained about the speech and writing of different social classes, and their child-rearing beliefs and practices, Bernstein's actual theory has not been tested at all.

This is not to deny the benefit of new knowledge about lower

class educational failure, or that Bernstein's theory has had a remarkable effect in stimulating such research, but the increase in sociolinguistic understanding has not been remotely proportionate to either the volume of, or practical gains from, research on restricted and elaborated codes.

The first stage of the test of Bernstein's theory is the more difficult task. From Bernstein's own writings about the theory it is not easy to imagine how the important variables, such as degree of restriction or elaboration, may adequately be either manipulated or measured. Indeed, to a naive reader, it might appear that Bernstein's theoretical passages are examples of 'meaning being implicit' (e.g. the 1965 opus).

Much of the important research at the Institute of Education of London University in the mid-1960s employed such measures as counts of 'rare words', type-token ratios, adjectives, of/in ratios and numbers of subordinate clauses. These are at best indirect operationalisations of the concepts Bernstein's theory embodies. It is easy to slide from 'elaborated code', defined sociologically, to 'elaborate language', defined in terms of form-class counts, which are not necessarily the same thing. Robinson (1971) states a basis for consigning utterances to elaborated or restricted codes with the virtue that it encapsulates in a phrase (emphasised below), much of the idea behind the distinction, yet which seems to throw one back upon one's intuition when trying to apply it:

> *It is important for an understanding of Bernstein's concepts not to invest the terms 'elaborated' and 'restricted' with all the meanings these words have in normal usage. They are the closest to the underlying ideas that we can find in the English language, but are far from ideal. 'Elaborated' is*

> *not to be interpreted as more complicated or more finely differentiated, and 'restricted' does not apply to all forms of constraint by user, usage, or situation. Utterances in 'elaborated' code may differ in their accuracy, level of analysis, or clarity, but are not rendered more or less 'elaborated' as a result of this. I think the most useful preliminary differentiating test is to ask whether an utterance functions to elicit or offer a proposition about the non-linguistic world, a statement which can in principle be accorded a truth value. If the primary focus is upon this referential usage, the utterance is probably in 'elaborated' code. Declarative statements and associated questions are the core of the 'elaborated' code. Such forms are also exploited in the 'restricted' code, but in this case the apparent and manifest purpose of the utterance will not be its true function. The 'restricted' code is concerned with control rather than information, prescription rather than description, commands and exclamations rather than statements and will contain questions which test affect and authority rather than fact.*
> (pp. 79-80, present writer's emphatics)

Clearly, to use this criterion in practice, one would rely upon the subjective judgement of a person or persons, whose objectivity could not be guaranteed, but this is possibly a type of case in which a speaker's intuitions given enough contextual information, might be better than measurements which were 'objective' in a mechanical sense.

It would be improper to leave the subject of sociolinguistic codes and class differences, without giving consideration to any results. Bernstein was by no means the first to examine class differences in language; previous work by Fries (1940) and Schatzman and Strauss (1955) although badly conducted in important respects, is discussed by Robinson (1972), who ruefully admits that methodological faux pas did not prevent Schatzman and Strauss from drawing what now seem to be correct conclusions.

Bernstein (1962, a & b) found that, in the upper reaches of the IQ range, verbal IQ scores for lower working class boys were lower than would be predicted from their performance IQs, whereas this did not

occur in a middle class sample. This appeared to indicate a subcultural ceiling upon their verbal skill development, consistent with their being limited to a restricted code. Further investigations with the same populations revealed less hesitation during utterances by working class boys (using the methods of Goldman-Eisler 1968) and class differences on a number of counts of form-classes, verb complexity and syntactic embedding, with group discussions of capital punishment as the raw material. To this author's knowledge, Bernstein has never shown how rowdy group discussions were submitted to Goldman-Eisler's automatic analysis for speech and silence. Such successful analysis necessitates high quality audio-recordings, without background noise and in which speakers are easily distinguished. Goldman-Eisler's own practice has been to use one speaker at a time with a throat microphone. However, the research has by now been corroborated many times over in its substantial import, using other methods.

Lawton (1968) a student of Bernstein followed up Bernstein's work, with less extreme social class groups, whose speech and writing were analysed in similar fashion. Lawton's results showed the same sort of social class differences as Bernstein's, but he had taken boys aged twelve and fifteen, so that the differences would be examinable at two age levels. The differences between social classes increased with age.

Robinson was meanwhile conducting research in the north of England, using 'formal' and 'informal' letters, written by twelve and thirteen year old boys and girls from middle and working class homes, all attending the same school (Bernstein's subjects were drawn from very different schools). He analysed these letters for the same sorts of lexical characteristics as Bernstein had studied and found social

class differences only for the informal letters, interpreting this as indicating differences in preference, rather than ability, between the social classes (1965b). In a slightly earlier report, Robinson (1965a) had used Cloze procedure to demonstrate differences between the speech and writing of middle and working class boys. His results showed that middle class boys used a wider range of words than their working class counterparts, with greater conformity of lexical choice in the working class group. Following this work, Robinson spent a year at Bernstein's London research unit and the subsequent research from both authors has increasingly stressed functional analysis of verbal interaction, which has thrown more light on the phenomena than the earlier, structural and lexical studies.

Before examining the later work, it is necessary to mention that a great deal of similar research was in progress across the Atlantic, largely under the impetus imparted by negro emancipation and the attendant wish to ensure that negroes benefited fully from the education process. Papers by Hess and Shipman (1965), Olim, et al.(1965) and Deutsch (1965), written with less emphasis on the Bernsteinian code concept, described a state of affairs in the USA similar to what had been observed in UK. Huffine (1966) failed to observe class difference in Arizona, but she analysed school essays which may have been interpreted even by lower class writers as 'formal' writings and hence been in elaborated code, as for Robinson's 'formal' letters. There was ample evidence that social classes in the Anglophone world differed in verbal dispositions, if not actual capacities, in accordance with Dr. Bernstein's theory of sociolinguistic codes and in association with educational success.

Bernstein's own main research report since the mid-60s (Bernstein and Henderson, 1969) concerned middle and working class mother's estimates of difficulty in teaching children skills or social mores, without the use of language. It was claimed that middle class mothers regarded language as important in the interpersonal domain, whereas working class mothers did not, although working class mothers seemed to regard language as more important than middle class mothers in the imparting of skills. One might be forgiven for predicting that the latter group would have assigned language more important in all situations, but the theory had altered by a sort of drifting process by the time that this paper was published, since the results were interpreted in terms of the working class possessing a Didactic and the middle class an Exploratory Implicit Learning Theory. This drift towards a more generalised, less specifically sociolinguistic theory of social class and education, seems to be one which would weld neatly with some of the results from research on working class authoritarianism (Eysenck, 1954, 1971) although the latter remains a very controversial field.

Robinson has recently turned his attention to the question and answer interactions of mother and child (Robinson and Rackstraw, 1967; Robinson, 1972) using the reports of mothers and children's answers to interview questions. The findings have been that middle class mothers give more answers to children's questions, answer more factually, more correctly and with more analytical and explanatory speech than working class mothers; middle class children were found to answer 'why' questions with reference to causes, consequences and interpersonal needs, while working class children responded in terms of authoritarian demands ('because it's bad') and regularity ('it never is').

The work of Hess and Shipman (1965), in USA, concerned black mothers and children in a teaching situation, using a game called Etch-a-Sketch, in which mother and child cooperated in reproducing designs. An index of specificity for mothers' instructions correlated highly with task success and showed almost horrifying class differences: working class mothers did not utter a single instruction more informative than 'Turn your knob'.

A report by Hore (1970) deserves special mention because it relates to one of Bernstein's predictions about the restricted code: that it would be associated with gestural and other non-verbal communication. Video-recordings of mother-child interactions in Canada were analysed and social class differences were observed, with the lower class mothers using more physical and the middle class mothers more ocular contact. This is not as ambivalent with respect to the prediction as might at first appear. The lower class physical contact fits well the didactic, positional control associated with working class, restricted code norms and the greater gazing by middle class mothers would be symptomatic of attentiveness while allowing their children to explore whatever they were doing without interference. It is also possible that eye-contact was an artefact of the middle class children's verbal productivity (cf. 1.2.2).

In evaluating the material outlined in this subsection, it is impossible to avoid the conclusion that Bernstein's hypothesizing about subcultural differences in language function has been impressively borne out by the facts now known, even if his sociolinguistic theory proper remains largely untested. Criticisms have been expressed by Labov (1970) and Coulthard (1969), who point out the indirect nature, mentioned earlier, of the link between Bernstein's theoretical terms

and his operationalisations, as well as the quantitative, rather than purely qualitative, differences between classes in the data. The force of these criticisms is logical, rather than empirical, and, as pointed out by Robinson (1972), it needs to be remembered that the facts have been uncovered in the process of testing quantitative predictions from Bernstein's qualitative theory, which is not an ex post facto explanation of a set of unanticipated quantitative data. The recent studies actually, through their concentration upon function more than structure, are more immune to the Labov-Coulthard criticisms.

An important theoretical paper by Greenfield (1972) has drawn a distinction between 'Oral' and 'Written' speech and discussed its educational implications in relation to cross-cultural cognitive research. 'Oral' speech is used to describe speech of a language which has never, or only recently, existed in written form and is characterised by being context-bound. 'Written' speech is speech of a language well-established in writing and tends to be context-independent. Greenfield points out that in a pre-literate society context-bound speech is usually quite adequate because speaker and hearer are almost invariably face-to-face. Her review of research from the United States, Britain and Africa relates oral speech to context-dependent thinking and instruction and she argues that context-dependent speech and thought are both more primitive than context-independent varieties. Education in Africa has the effect of making children more context-independent and hence analytical and personalised in their cognition. Her own research showed that learning of a written language caused children's use of an oral one to become less context-bound and she cites an unpublished finding by Cole, Gay and Glick that of two Liberian tribes the Vai, who had an historically indigenous written

language, were better able to communicate non-visually than the Kpelle. It is strongly tempting to assimilate Greenfield's terms to those of Bernstein, yet to do so would probably mean losing valuable meaning particular to one or both formulations. Undoubtedly, Greenfield's observations are highly relevant, and especially so when it is recalled that the number of generations of literacy in the British and American working classes is probably not more than four on average (compulsory schooling in the United Kingdom is just over a hundred years old) and that most of today's elderly working class people learned to talk in extended families with a high proportion of illiterate adult members. An adequate emerging theory will presumably incorporate the valuable aspects of both Bernstein's and Greenfield's formulations.

1.3.5 Speech as an interpersonal cue.

Pear (1971), pointing out the wider variety of class and regional speech styles in UK than USA, has argued that the greater popularity of radio plays in the former country may have something to do with the respective possibilities for conveying social information through (non-content aspects of) speech. Even in a society less heterogeneous than Britain, the possibilities for conveying social cues by, accent, through dialect, 'register' and so on, are considerable.

Lambert, at McGill University, developed 'Matched Guise' technique, for studying effects of speech variations upon social perception. Recorded voices of one or more speakers are heard saying the same words in more than one guise, i.e., accent, language, sociolinguistic code, etc. Very often all guises are heard by each

listener who judges them usually on bipolar trait scales. Guises are matched in the sense that the speaker behind the voice remains constant and this allows more experimental control than would be possible if, e.g., 'genuine' speakers with different accents were employed. Lambert (1967, p.100) claims of the technique that it 'rather effectively calls out the stereotyped impressions that members of one ethnic-linguistic group hold of another contrasting group'. The matched guise technique has been used with success in Canadian and British work, but it possesses two flaws, easily overlooked in the face of its convenience and efficiency.

One of these will be considered after discussion of some results, the other flaw is that languages, even more codes and accents, are not easy to vary independent of other social cues. A wag once boasted that he spoke German to his dog, Spanish to his horse, English to his wife and French to his mistress, illustrating how languages vary in convenience for different kinds of subject matter. A particular language, or accent, may not normally be used when the topic is 'neutral', or else is normally discussed with a certain register. The 'warm' accents of northern England may sound warm more because of accompanying sounds and non-vocal signals than features of the accents themselves. If so, a competent speaker might either be unable to vary guise independent of such correlated cues, or when doing so be perceived in a misleading way because of the unfamiliar combination of accent, register, etc. In effect, a discrepancy between the systematic, subjective perceptions of the listener and the objective characteristics of the speaker could occur, as with phoneme perception (Liberman et al. 1967). Ervin-Tripp (1967) has reported that Japanese wives of American ex-servicemen find English and Japanese convenient for discussing quite

different sorts of content, suggesting that registers may not overlap greatly between the languages. In fairness to Lambert and fellow-workers, there has been little indication of their work being hindered by this methodological flaw and they might contest that it is a flaw at all, on the ground that they study connotations of guises which could not be properly investigated if the kind of independent manipulations this author advocates were carried out. Such would be a doubtful argument because the fundamental reason for using matched guise technique if after all to establish more independent control over guises.

The research by Lambert and others with the technique has yielded plenty of results. McGill research reviewed by Lambert (1967) has been carried out on French-English bilingualism in Quebec, with fluent bilingual speakers. English Canadian speech has been found to elicit generally more favourable judgements than French Canadian speech, whether listeners are French or English Canadians, and Continental French speech elicits favourable judgments from both Canadian communities. Women of both ethnic groups were 'loyal' to men of their own communities though men were less 'loyal' to their women. Subsequent developmental studies of sociolinguistic stereotypes indicated age twelve as fairly crucial for girls, and more for middle than for lower class girls, who showed bias less and of more transient character. Other work by Lambert's team has used Arabic and Hebrew guises and accent switches between Standard American English and Jewish English, and Ashkenazic and Sephardic Hebrew. Lambert is particularly concerned with the influence of sociolinguistic attitudes upon foreign language learning, an important field too specialised for present consideration. Of general interest in Lambert's work is code-switching by individuals, which will be considered under 1.3.6.

Similar research has been carried out by Triandis et.al. (1966) and by groups in Britain and Africa. Triandis varied the race and dress (by photograph), attitudes (pro- or anti-racial integration) and speech ('excellent' or ungrammatical) of a recorded speaker. Language accounted for about seventy-five per cent variance on semantic differential factors of friendship, evaluation and admiration, but only thirty per cent variance on a social distance factor, of which the main determinant was race. Although the authors a are vague about directions of effects, having been concerned with relative influences of the independent variables, rather than directions, it is implied that 'excellent' English elicited favourable judgements.

The main British research has been carried out by Giles (1970, 1971, a & b), with a cameo piece by Cheyne (1970) and groundwork by Strongman and Woosley (1967), who used Yorkshire and 'London' ('cockney' not specified, so middle class London presumed) guises, produced by two speakers. Subjects from the north and south of England heard recorded voices and rated speakers on eighteen trait scales. Both groups displayed similar stereotypes of the two guises, with no differences between regions of subjects' origin, although the fact that subjects were all students at a single university would tend to minimise such differences.

Cheyne's experiment was along similar lines, but using English and Scots guises and subjects, drawn from varied occupations and residing in their home countries. Differences were more pronounced for male than female speakers, English voices communicating prestige and competence, but with Scots subjects rating Scots voices higher on sociability traits.

Giles (1970) began by finding reliable differences in prestige accorded to various regional accents and continued (1971a) with South Welsh and Somerset secondary pupils as subjects, presenting South Welsh, Somerset and Received Pronunciation recordings, by two speakers. Received pronunciation was again attributed greater competence and the regional accents integrity, humour and social attractiveness, with considerable local accent loyalty being manifest. Giles (1971b), administered the British Ethnocentrism Scale (Warr et al., 1967) and found, as he predicted, that persons high on ethnocentrism showed bias towards Received Pronunciation and against regional accents.

The results to form a consistent picture which needs no spelling out, but all these studies, and those of Lambert are subject to the criticism, that what may actually happen in experiments of this sort may be that listeners attend to voices only enough to identify speakers as belonging to particular ethnic groups and then respond merely on group stereotypes. This forms the second of the two flaws in matched guise research previously referred to. To this writer's knowledge, no efforts have been made to control for response to general stereotypes, yet this would be elementary and simple. If it were found that social judgements based on speech samples showed no differences from judgements based on group names like 'Welshman', 'French Canadian', etc., matched guise research would not be advantageous. Even if there are differences, they should not be assumed necessarily to reveal anything not already known from experiments on impoverishment of social cues. We might expect recorded voices to be less impoverished information than mere group names, but more so than video-tape recordings of speakers or face-to-face confrontations between speakers and listeners.

An experiment similar in some ways to the above, but to which the criticism is probably inapplicable, partly bears out the 'local loyalty' noted by Giles. It was carried out in Uganda, where clear social stereotypes of the voices used would not be very likely among the industrial workers who were listeners, by Scotton (1970). She presented different recorded guises of Kiswahili, a lingua franca in East Africa, to Africans of Ugandan and Kenyan origin dwelling in Kampala, where the local Kiswahili is a debased variant. Scotton's concern was more with subjects' knowledge of the Swahili language than social bias, but results still reflected the latter. Immigrant Kenyan workers, from regions nearer the coastal Swahili homeland, were, not surprisingly, found to be more accurate than Ugandans at distinguishing Standard Swahili from 'Up-Country' speech. More to the point, Baganda, members of an advanced tribe living around Kampala, with very proud, conservative attachment to its cultural traditions, were particularly prone to perceive the local variant as correct and the true standard as incorrect, even though local Swahili is to them a foreign and scorned tongue.

Speech has always tended to be a very decisive cue to social identities of importance in encounters: tribe, caste, religion (cf. Matthew, Chap. 26, verse 73). The weekly allegations in the press, that Britain, Australia or America has a rigid, class- or caste-ridden social structure, with strong barriers to social mobility, reflect more than anything else the writers' ignorance of the non-Western world, where far more extreme forms of these divisions are accepted as the ordained way of things, not worth remark, let alone criticism.

An effect of colonial experience has been disturbance of such social stability and as a result, changes in the relative statuses of languages. Thus have French, English and some local languages (e.g., Swahili in East and Hausa in West Africa; Tagalog in the Philippines and Hindustani in India) become markers of prestige, not always without serious political repercussions. Some dilemmas of the speakers involved as illustrated by Alexandre's (1971) account of language use among Cameroonese elite families, would themselves be a fitting subject for research.

This review of some research on speech as an interpersonal cue is longer than it would have been, largely because matched guise technique was used in Experiment III of the work to be reported. It is hoped that possibilities and limitations of the method may be more obvious through such relatively extended treatment.

1.3.6 The influence of situation

The term 'situation' is used here in the sense of a transient rather than long-term state of affairs, following the usage of Hymes (1964), which is rather similar to that of Goffman's term 'Setting'. In a paper entitled 'The neglected situation' (1964), Goffman wrote:

> *... it is the greasy parts of speech that are now increasingly being considered. A wagging tongue (at certain levels of analysis) proves to be only one part of a complex human act, whose meaning must also be sought in the movement of the eyebrows and hand. However, once we are willing to consider these gestural, nonwritable behaviours associated with speaking, two grave embarrassments face us. First, while the substratum of a gesture derives from the maker's body, the form of the gesture can be intimately determined by the microecological orbit in which the speaker finds himself. To describe the gesture, let alone uncover its meaning, we might then have to introduce the human and material setting in which the gesture is made.* (p. 133)

The simplest thing one could measure about speech would be quantity and this has been done, by Argyle et al. (1968), who varied the visibility of persons in interviews and conversations, as was done in Experiment I of the present work. They found that concealed subjects spoke more than unconcealed subjects, averaging ninety seven and seventy seven words per minute respectively and that males spoke more than females, in a ratio of 95 : 79. An interaction was significant, with males speaking still more when invisible and addressing females. It is not apparent that the authors separated the overall amount of speech in a situation from the balance of the floor between speakers, although this would be quite practicable. If a conversation occurred, in which no periods of silence lasted more than, e.g. one second, the verbal productivity of either speaker would be almost completely tied to that of the other, whereas when fairly long silences are interspersed between utterances, each person is more free to choose his own level of verbal productivity (it is here assumed that simultaneous utterances do not happen, which is approximately so in most conversations).

Jourard and Friedman (1970) used verbal productivity as a (very dubious, but present irrelevant) operationalisation of Self-Disclosure, varying the physical presence/absence and eye-contact of an interviewer. They found women spoke only half as long to an interviewer as to a tape recorder, but that males did not vary and that eye-contact had no effect. Since only one, male, interviewer was used in the experiment, it is not possible to interpret fully the subject sex difference obtained.

Moscovici (1967), in a review of speech in social interaction, mentions one experiment in which effects of pressure to reach a

conclusion and pressure to do so within a fixed time limit for discussion were compared with a control condition, on several dependent variables. It was found that less speech occurred under either of the two experimental conditions than under the control condition.

Other results from Moscovici's experiment were based on analysis of the type-token ratio and counts of new words in successive thirds of the discussions, all three dependent variables being estimates of lexical redundancy. His predictions included, inter alia, that pressure to reach a conclusion would lead to narrowing of vocabulary whereas the control group would show increasing introduction of new words, as conversation ranged more widely. The time pressure condition was expected to yield a steady, high rate of new words as conversation drifted from topic to topic. All predictions were impressively confirmed. Whether because of difficulty with English or not, Moscovici, who works at the Université de Paris, succeeds in giving the impression that his predictions were made post hoc, in which case their accuracy appears less surprising, but it is hoped one-tailed significance tests were not used:

> *In the third situation, where external pressure was imposed, the partners were given the impression of not having enough time. They did not become involved in a real conversation; all their exchanges were modified by their preoccupation with the experimenter's impending order to stop. Each person tried to say something, responding to his partner only in order to cope with the situation. We would therefore expect...* (pp. 245-6, present author's emphasis).

A second experiment Moscovici reported in the same review and particularly relevant to Experiment I of the present series, involved manipulation of orientation and visibility in dyadic conversations.

Face-to-face, with and without a screen separating the subjects, side-by-side and back-to-back arrangements were employed and counts of Nouns, Verbs and Function Words were analysed. Side-by-side and Back-to-back conditions both showed more functors and nouns and fewer verbs than the face-to-face orientation, but the Screen condition did not differ significantly from the control. Moscovici argues that Back-to-back and Side-by-side are both unfamiliar positions for conversation, whereas placing a screen between speakers may not be, though he is not fully clear why this should be so. He maintains that it is the social relationship rather than the physical circumstances which determines syntactical choices, although syntax was not in fact covered by his analysis. Moscovici claims, and others accept (e.g. Robinson, 1972) this experiment showed that (p. 259) 'On the whole, verbal output of the back-to-back and side-by-side participants resembled written language', but since no statement about known differences between spoken and written language is made, this is not proven. It would also be interesting to see what would have happened, had Moscovici's subjects been able to practise discussion in the supposedly unfamiliar circumstances until they became familiar with them. What the experiment may do, if Moscovici's interpretation of the difference between the back-to-back and screen conditions is accurate, is provide evidence for the assessments of both he and Robinson (1972) that non-verbal signals play a negligible regulatory role in encounters.

Loewenthal (1968) required subjects to pass written messages describing coloured shapes through a slit in a screen to a confederate, who, posing as a fellow-subject was supposed to identify them from an array. It was found that when the confederate's shape selections

were primed to indicate failure to understand messages, subjects increased their message lengths and used more function words, though the proportion of lexical words was unaffected. The obvious comment upon this experiment is that the procedure, with all messages in writing, seems unnecessarily artificial and that the experiment could easily have been done aloud, with greater confidence that results would generalise to situations outside the laboratory. Loewenthal does not mention Bernstein in discussing her results, but it would be predictable from Bernstein's theory that when the recipient of a message shows signs of not understanding the sender would be likely to switch to a more elaborated code for subsequent messages. Increase in functors cannot be taken as conclusively indicative of code elaboration, but the overall increase in message length, suggests that subjects took greater care to be precise.

Sociolinguistic switching, including code-switching, is a potentially very productive area. An undergraduate may be on first-name terms with certain of his tutors, but he will vary his mode of address, according to situation: if he has any social insight, he will not call a young don by his first name in the presence of the vice-chancellor. Most persons address each other more formally before subordinates.

Ervin-Tripp's (1967) study of Japanese women married to American males in USA provides evidence that the topic of conversation may be a situational determinant of language-choice. Ferguson (1966) has reported that Berbers choose their vernacular only for intimate, domestic conversation and Arabic for most other uses, whereas Kurds speak Arabic as a lingua franca and for discussion of religious truth,

using Kurdish for all other purposes.

Gumperz (1964) and Blom and Gumperz (1972) have reported observations in Hemnesberget, an old settlement in NW Norway now being affected by industrialisation. Their evidence is that the local Ranamål dialect is preserved, in the face of increasing exposure to standard Norwegian Bokmål, because the codes, although very alike by objective linguistic standards, fulfill different functions for natives of Hemnes. Ranamål marks the in-group and reinforces its felt separateness from the industrial workers who settle there.

> *On one occasion, when we, as outsiders, stepped up to a group of locals engaged in conversation, our arrival caused a significant alteration in the casual posture of the group. Hands were removed from pockets and looks changed. Predictably, our remarks elicited a code switch marked simultaneously by a change in channel cues (ie. sentence speed, rhythm, more hesitation pauses, etc.) and by a shift from (R) to (B) grammar. Similarly, teachers report that while formal lectures - where interruptions are not encouraged - are delivered in (B), the speakers will shift to (R) when they want to encourage open and free discussion among students. Each of these examples involves clear changes in the participants' definitions of each other's rights and obligation.* (Blom and Gumperz, 1972, p.424)

It is interesting to note a non-verbal switch accompanying the verbal one, in the first example.

Blom and Gumperz point out an interesting distinction between situational switching, as instanced above, and what they call metaphorical switching, in which phrases and statements in Ranamål are interjected into conversations between in-group members for reasons of content (they cite an example of town hall business). It was noticed that greetings and parenthetical exchanges about non-business content involved dialect-switching. Such examples also occur in truly bilingual contexts and, in the present writer's

experience, invariably involve switches to the in-group language for the interpolation of remarks, which, using Robinson's criterion of social control, as opposed to transmission of information, would be categorised as restricted code:

> *'It's not the real Mackay,' he commented, as he offered his guest one glass and took the other for himself. 'A dhuine, dhuine, no, indeed. Still it's a little better than Joseph's ginger-ale and much better than his lemonade.' The priest raised his glass. 'Ceud milé failté, agus slainté mhor!' Then he drained it in honour of his guest and put it down with a wry face. 'Ah well,' he sighed deeply, 'we're in a pretty bad way.'* (Whisky Galore, by Compton Mackenzie)

In the above example, Father Macalister switches to the Gaelic for a phrase which adds pathos and emphasis ('A dhuine, dhuine,' = 'O man, man') and to express welcome and raise a toast (Ceud milé failté, agus slainté mhor,' = '100,000 welcomes and great health'). The present author observed similar switching when installed in a university office in which the telephone had by tradition been used by nearby African secretarial staff. Apart from switching from English to Luganda for passages the author was meant not to overhear, they usually gave the lengthy, ritualised greetings in vernacular which they also used for interpolated phrases familiar to the author connected with social control.

At the phonetic level, Labov (1964) has investigated production of different allophones of /r/, /θ/ and other phonemes, in relation to both social status and situation, in New York. Within what appears to be a single phonetic system, speakers vary in their realisations of such phonemes: 'three cars' may be rendered as 'tree cahs', which as one would expect is more common among the lower than the upper social strata. However, Labov also found upon varying

the situation from a casual type, through increasingly formal levels, to the pronunciation of minimal pairs, like dock and dark that subjects of all social classes showed progressively higher percentages of the Standard form. An extremely interesting finding was that the lower middle class over-corrected in more formal situations (see Figure 1.4). The obvious explanation for this finding would be that the lower middle-class is insecure about its social status and aspires to a higher one.

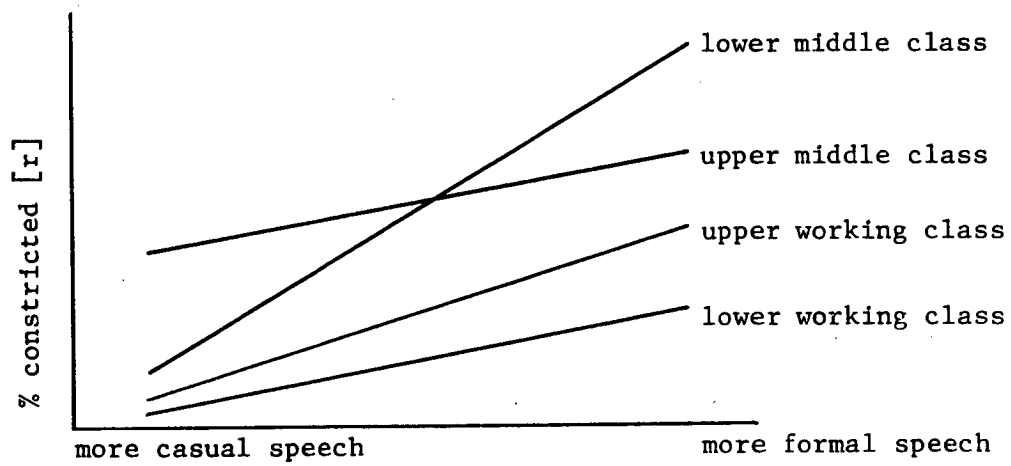

Figure 1.4 Simplified illustration of class stratification of post-vocalic [r] in New York (after Labov, 1970)

Gumperz's research (Gumperz and Hernandez 1971) has lately involved switches within utterances between languages more than the earlier Hemnesberget reports. The subjects have been Spanish-Americans and attention has focussed, not upon Spanish intrusions which are merely part of the speakers' customary style of English, but on cases where a clear switch from one code to another occurs. Gumperz and Hernandez find constraints at the syntactical level on where switching may happen and what units of speech may be switched. They

note favour for English with certain types of concepts (e.g., 'psychological' terms, like 'pacify'), which indicates semantic constraints upon switching. Speaking Spanish to mark intimacy or ingroup relationships was noted, e.g., a switch to Spanish being made to discuss a third person of Spanish-American group membership. In interpreting their data, Gumperz and Hernandez draw an analogy between their subjects' use of Spanish and the use by those of Brown and Ford (1961) of first-names, claiming:

> *'English forms ordinarily associated with non-members, i.e. non-chicanos, are like familiar pronouns, in that they convey secondary meanings of solidarity and confidentiality.'* (p. 124)

A valuable aspect of the Gumperz and Hernandez study, which has actually revealed little that could not previously have been guessed, is that their data-gathering involved less questioning of respondents and more observations of spontaneous speech than usual. Much questioning of respondents which has been carried out in sociolinguistics should have been backed up by more data on spontaneous conversations. In sociolinguistics, where rules are probably more like cultural and moral ones than rules of pure grammar, self-reports and intuitions may be less reliable than in the study of syntax and phonology. Social relationships and situations are very finely differentiated and, when asking which usage is appropriate in a given context, the researcher is more likely to be greeted with 'Well, it all depends on ...' than if asking whether one should say [a apple] or [an apple].

1.3.7 General comments on sociolinguistics

The foregoing coverage of the sociology and social psychology

of language carries little discussion of theoretical aspects, because, as previously stated the present state of the field displays no outstanding theoretical issue upon which to focus. The occasional sweeping hypothesis, such as Whorf's and the attempts at taxonomies described, as well as other similar work, constitute much of the theoretical side of sociolinguistics. A large proportion of the literature which might lay claim to be of a theoretical nature, like so much other social science theorising, is concerned with devising and illustrating terminology, offering distinctions which may be drawn. Bernstein proposes the distinction between elaborated and restricted codes; Greenfield that between oral and written language; Fishman (1970) distinguishes between macro- and micro-level sociolinguistic analyses and between bilingualism and diglossia. Inevitably some of these will fall into disuse, while research will crystallise around others, but which will be which it is not possible to forecast at the present stage. Nor is there much point in attempting to make such prophecies, because the theoretical future of sociolinguistics is in a literal sense undetermined and will take whatever course the enthusiasms of researchers, among other influences, urge. Hard data, although the ultimate test of theory, is by no means its sole origin, if it is a source of theory at all.

About the kinds of properties sociolinguistic theory should have something may be said. The theory of grammar has made its recent strides with the almost exclusive study of structure. Robinson (1972) is one who, while acknowledging the importance of structural studies, stresses the necessity to examine language functionally. It would be tempting to imagine that sociolinguistics was the functional counterpart of generative grammar and this is not wholly untrue, but the structures

for which sociolinguists seek functions are not necessarily the same as those studied in generative grammar. What determines choice of the passive voice is likely to be more immediately semantic in nature than social (Johnson-Laird, 1968). Sociolinguistic structures exist in their own right to be explained, in the form of associations and causal connections between social and verbal phenomena and these include the observations which have been reviewed. It will be necessary to describe sociolinguistic structures in terms of sociolinguistic rules -- this is part of the nature of social, as contrasted with natural, science though close parallels between sociolinguistic rules and the rules of transformational grammar may be misleading -- and such sociolinguistic rules themselves will form higher order structures demanding further explanation, which could well be functional, in terms of social survival functions for types of rules.

Isolated functions as explanations of isolated events do not carry much scientific conviction and a functional theory presupposes taxonomical sophistication, so attempts such as those of Robinson to develop this are to be welcomed , for their value in fitting both structures and functions into non-arbitrary categories. The medical, biological and geographical sciences are enviable, with terminology acceptable to investigators of widely differing persuasions, but the same is not true of most social sciences where arbitrary systems compete, with few principles established for choosing between them. Might there be some value in the historical study of how taxonomies emerged in these other sciences? Quoting Kroeber, Hymes (1967) wrote:

> *...anthropologists ... do not yet clearly recognize the fundamental value of the humble but indispensable task of classifying - that is structuring - our body of knowledge, as biologists did begin to recognize it two hundred years ago.* (pp. 11-12)

Almost any taxonomy is better than none and it is yet possible that the diverse findings on social class, accent and person perception, modes of address and language choice will begin to make some related, overall sense if one emerges in sociolinguistics.

In the present chapter, a variety of research has been touched upon, most of which is not immediately relevant to the experiments carried out by the author himself. The next chapter reviews experiments which bear more directly upon the specific subject of his research, yet it is intended that Chapter One set the scene for what follows in illustrating how speech is neither the mere generation of syntactic structures nor a monotonous babble accompanying non-verbal communication. A conversation is the interweaving of communication by body and speech, with each of these aspects able to support or conflict with the other. Just as in the Motor skill model of social interaction proximity and eye contact may substitute for one another, verbal and non-verbal communication may likewise be adjusted to achieve desired social equilibria. Intimacy may be increased by a change of language or code just as by kinesic means. At the centre of organisation of each social performance lies the human mind, adjusting bodily and aural messages to keep the main transactions of the occasion in motion by regulating the floor, the emotional intensity and the pace of the exchange through ancillary signals. Most of the main content passes through the non-verbal channel, but there are moments when the channel functions are reversed and there are occasional events and

phenomena which form some of Goffman's 'greasy' aspects of speech and which serve as reminders that the difference between verbal and non-verbal channels is more a logical than an empirical one. Among such events and phenomena are the vocalisations and gaps in vocalisation which, while omitted from most books on language and rarely appearing in written language form an intrinsic part of every spontaneous utterance. It is with these that Chapter Two is concerned.

CHAPTER TWO

NON-LINGUISTIC ASPECTS OF SPEECH

NON-LINGUISTIC ASPECTS OF SPEECH

The late T.H. Pear, writing in 1930, announced a word suggested to him by a professor of Latin for facility in 'deliberate, adequate, verbal expression': Euphasia. Fortunately or otherwise, it has not been adopted by psychologists of speech as a technical term, but it is an apt description of the opposite of what this chapter is about, for it is intended to review here some findings about 'speech disruptions', the breaks, repetitions, and intrusions, which occur frequently in normal speech.

Non-linguistic aspects of speech can conveniently be grouped under the two headings, Paralinguistic and Extralinguistic, although they are sometimes used almost interchangeably. For present purposes, paralinguistic may be taken to mean such features as stress, pitch, speed and intonation, which, although not part of the verbal message itself, are inseparable from it and may add nuances to the verbal content. Extralinguistic features are presently defined as features extrinsic to the verbal message intended by the speaker which could, in principle, be subject to such a 'cleaning-up' process, as is done to a parliamentary Hansard, leaving a message grammatically consistent and erasing extraneous vocalisations. Whereas paralinguistic features would not normally be present in a written transcript, a strictly verbatim transcript would include extralinguistic features like uncompleted words and phrases, stutters, repetitions and incoherent noises, as well as, perhaps, symbols for long gaps. Concern will presently be mainly with extralinguistic features and in particular with the Filled Pause.

2.1 FEATURES OTHER THAN THE FILLED PAUSE

2.1.1 Silent hesitation pauses

Among the best-known research in this category is that of Goldman-Eisler on hesitation pauses, summarised in a book (1968), representing about fifteen years' papers published in several journals and carried out at University College London. She has used electronic equipment for analysis of speech and silence to investigate the significance of speech rate and silent hesitation pauses.

Typically, a subject in one of Goldman-Eisler's experiments is required first to describe the content of, and then to summarise and interpret, a set of cartoons which take the form of short sequences of pictures without text. Frequencies and durations of silence relative to words or seconds of speech and rates of emission of words per second are among the measures studied. Goldman-Eisler has conclusively demonstrated that silent pauses occupy a considerable proportion of total speech time (thirty five to sixty seven per cent, in her data), that they tend to occur before unpredictable words, both between and within phrases, that they occur more often and are longer when the subject is required to think (i.e., interpretations, as opposed to descriptions) and that this effect is not a direct function of syntactical so much as semantic decisions.

In further work, Goldman-Eisler has investigated the effects of a cortical depressant (chlorpromazine) and a cortical stimulant (amphetamine) upon speech pauses. She found that the difference between descriptions and interpretations, in pause-time per word was increased for some, and decreased for other subjects, by chlorpromazine, whereas amphetamine fairly uniformly reduced the difference. Her

interpretation is that chlorpromazine interacts with a personality variable, which, drawing upon the physiology of Hughlings Jackson, (1878), she labels tonicity, but, rather unfortunately, does not attempt to relate to conventional trait-terms in modern psychology. Two things need to be noted about this experiment and the first applies also to others by Goldman-Eisler: the number of subjects involved was small (ten) and secondly, there is a substantial correlation to which she does not draw attention between the absolute magnitudes of the effects of chlorpromazine and amphetamine upon the hesitation measure she used. The value determined by the author for this is $r = 0.74$, just short of the one per cent significance level. It would therefore appear that tonicity, or whatever it is that interacts with chlorpromazine to affect a speaker's hesitancy during a difficult verbal task, also relates closely to amphetamine susceptibility.

The evidence from Goldman-Eisler's work, both with and without drugs, is that silent hesitation assists production of 'euphasic' speech and she argues that tonicity may be understood as the speaker's preparedness to delay vocalising, while he plans ahead. It is interesting to note that Ramsay (1966), using six different verbal tasks, including reading and conversation, reported relationships of mean speech-lengths, mean silent pause-lengths and ratio of total speech to total silence with extraversion, measured by Eysenck's Personality Inventory: Introverts tended to give short utterances with long pauses; extraverts long utterances with short pauses. Ramsay's paper reports no means for extreme groups he compared by analysis of variance and fails to show any analysis at all for the conversation task, promising to do so elsewhere. Despite these faults, and the fact

that only twenty three subjects participated, Ramsay's report concurs with Sanford's (1942) views on the subject of differences between extraverts' and introverts' speech. A later experiment by Ramsay (1968), employing fiftysix students at a Dutch university, who again performed six similarly varied verbal tasks, substantiated and added to his previous findings but only for the mean silence measure. He found that the difference between extraverts and introverts on mean silence length is a function of task, being greater for more complex tasks than for reading. Neuroticism did not relate consistently to the speech measures. Differences between tasks, on Ramsay's experiments lend support to some of Goldman-Eisler's results on hesitation and task difficulty.

Mention should be made of an extensive series of experiments by A.W. Siegman and B. Pope, of the University of Maryland, who have experimented on several hesitation variables, mainly relevant to sub section 2.1.2, but including a Silence Quotient, the proportion of total speech time spent in pausing. Their usual criterion for a silent pause is a minimum of two seconds. Using analogues of the initial psychiatric interview, mostly with normal subjects, they have consistently found more or longer silent pauses in responses to ambiguous questions. In an experiment (Siegman and Pope, 1966a) in which TAT responses were used, however, they failed to find a higher silence quotient for highly ambiguous material than low ambiguity pictures, cards of medium ambiguity having a significantly lower silence quotient than either of the other categories. This is not what one would expect, in view of Goldman-Eisler's results and their own in interviews, but in this experiment a three second minimum was set for the silent pause. About ninety per cent of Goldman-Eisler's pauses were less than three

seconds in duration, so comparability of the results is doubtful, although, since Goldman-Eisler's short pauses would reflect in Siegman and Pope's Articulation Rate, as the authors point out, an increase in short pauses would be recorded as a decrease in articulation rate in their experiment, which did occur. Siegman and Pope state that, according to Goldman-Eisler, only longer pauses, over three seconds in duration, are affected by uncertainty and argue that there is a genuine discrepancy between their and her results. Actually, Goldman-Eisler's data (1961b, p. 233) show that the difference between frequencies of pauses in descriptions and interpretations emerges with pauses between two and three seconds long, so that her data are not contradicted by those of the American team, after all, since there is an overlap of their articulation rate scores with her two-to-three second pauses, which do reflect cognitive effort.

In two other experiments Siegman and Pope (1965b, 1966b) have found substantial negative correlations ($r=-.29$, $n=50$; $r=-.59$, $n=50$) of the silence quotient with Eysenck's Extraversion scale, thus corroborating Ramsay's results.

A number of investigators have studied silent pauses in relation to either their linguistic or situational contexts. In a seminal paper on the subject, Maclay and Osgood (1959) reported an analysis of over fifty thousand running words of speech. They observed positions of occurrence for a number of distinguishable hesitation phenomena, in relation to surrounding form-classes of words and sentence-frames derived from work by Fries (1952) and found that, in their sample, silent pauses occurred more often before Contentives than Functors, which would be consistent with Lounsbury's (1954) hypothesis that they precede points of high statistical uncertainty.

Cook (1971b), in a paper to be mentioned again later, points out that Maclay and Osgood failed to control for relative frequencies of functors and contentives and that their speech sample was preselected for representative grammatical sequences. Boomer (1965) argued that previous research on hesitations had concentrated upon their relation to particular words, as though speech was uttered with the word as the main unit. He pointed to another aspect of Lounsbury's (1954) hypothesising, that hesitations precede encoding units, and suggested that speech is encoded in phonemic clauses, defined by Trager and Smith (1951) in suprasegmental terms of stress and terminal juncture (i.e. not in terms of syntax). Boomer found that hesitation pauses occurred markedly after the first words in phonemic clauses, interpreting his data as supporting an hypothesis that the phonemic clause is the unit of encoding. Boomer did not include juncture pauses (i.e. pauses at grammatical breaks) in his silent hesitation category. Had he done so, it is likely that hesitations would have occurred more before the first word in a phonemic clause, as, although phonemic clauses are suprasegmentally defined and scored, their boundaries often coincide with grammatical ones. A paper by Hawkins (1971) reports that hesitations also occur mainly at beginnings of syntactical clauses, but he did not separate filled from unfilled pauses, so it is impossible to draw much from his finding alone, especially since Boomer found filled more than unfilled pauses to occur before the first words of phonemic clauses.

It is unfortunate that, with respect to silent pauses and other hesitation phenomena, different investigators have frequently used incomparable, or barely comparable measures. Goldman-Eisler and Ramsay opted for strict objectivity by letting hardware

distinguish between speech and silence with fixed thresholds for volume and duration, but it is easy to see that irrelevant noises, such as coughs, nose-blowing or movement of furniture, could be registered as speech, unless carefully pre checked by humankind, and that long juncture pauses may be used for emphasis, whereas true hesitations of only a few milliseconds duration could occur, so that a fixed criterion (e.g. Goldman-Eisler's 0.25 seconds) is bound to lead to error. More linguistically oriented researchers have preferred subjective criteria, on the implicit basis that a native speaker can spot accurately a true hesitation pause.

Three experiments by Tannenbaum and his associates are worth mention. Tannenbaum et al. (1965) used Cloze procedure to study the predictability of words surrounding hesitations, identified by trained coders as 'silences of unusual length', explicitly to avoid emphatic pauses, etc. Unfortunately, they did not separate silent from other hesitations in their analysis. Not only did their data support Goldman-Eisler's finding of low predictability for words immediately succeeding pauses, but, they reported equally low predictability for words immediately before pauses, using five partly interrelated measures of predictability. An earlier experiment (Stolz and Tannenbaum, 1963) showed greater pause time when a subject, subjected to a university oral examination under the bizarre conditions of being alone and speaking to apparatus while being viewed through a one-way screen, received disconfirmatory feedback about his efforts. The third experiment (Tannenbaum et al. 1967) involved factor analysis of ten fluency measures, yielding four factors, with silent pauses loading on one, in company with speed of speech and ratio of functors to contentives. This factor was interpreted simply as fluency.

Taken as a whole, the work by other researchers modifies very little the cognitive implications of Goldman-Eisler's, although her lack of rigour in sampling and inconsistency in reporting (compounded by numerous misprints in her book) make extra evidence welcome. Speakers hesitate when they need to make unpredictable semantic selections, to some extent at the level of individual words but also at the level of short phrases, defined or not in syntactic terms. The more a person is prepared to pause, the more elegant the verbal (though not always the extralinguistic) expression he utters, and the marked individual differences are a function of personality.

In view of the fact that Eysenck (1957) conceives the difference between introverts and extraverts to lie in the introvert's greater cortical excitation, and therefore tolerance of delay in the discharge of impulses, there would appear to be every reason to regard extraversion-introversion as the personality variable associated with Goldman-Eisler's tonicity differences, although this raises further questions about the effects of chlorpromazine and amphetamine.

2.1.2 Miscellaneous hesitations and stress

When Goldman-Eisler began her work in London, George Mahl at Yale University was also starting to study hesitations and what he called 'speech disturbances', hoping to develop convenient and unobtrusive behavioural measures of anxiety in the clinical field. He presented exploratory findings which indicated the programme's feasibility in 1956, identifying eight categories of disturbance:

Ahs, (i.e. filled pauses) Sentence Corrections, Sentence Incompletions, Repetitions, Stutters, Intruding Incoherent Sounds, Tongue-slips and Omissions.

Mahl also used a Patient Silence Quotient, similar to that of Siegman and Pope. Reporting that about thirty interviews from twelve patients had been scored for the dependent variables described, Mahl presented details from two patients who had produced most of the speech studied. The patient silence quotient and a Speech Disturbance Ratio obtained by dividing total number of scored speech disturbances of all kinds by number of words spoken both showed signs of rising substantially, from phases of the interviews rated as non-anxious to those rated as anxious, by independent judges working 'blind', but limitations of unrepresentative samples necessitate caution about the result's significance.

It was not long before Mahl (1959) refined his anxiety estimate, discovering that Ah was different from the other speech disturbances, actually correlating negatively (r=-.30) with the anxiety measure. Before leaving Ahs to section 2.2 on the filled pause, it is worth noting that this correlation implies that the filled pause alone is still almost as good an estimator of anxiety as all the other disturbances put together, which correlated with the anxiety measure only +.36 overall and which showed disturbing inconsistency between the sexes in this relation: for twenty females, r=+.59; for eleven males, r=-.47. Such a difference casts doubt on the validity of the NonAh speech disturbance ratio as an indicator of anxiety. Fortunately, other evidence strengthens its case.

In the work just described, Mahl measured situational anxiety,

not anxiety as a permanent, personality characteristic. It is important to distinguish between the two, as either could relate to speech disturbances, without implicating the other at all.

2.1.2.1 Situational Anxiety. A number of experiments in which situational anxiety and related variables have been manipulated provide support for the hypothesised relationship between NonAh speech disruptions and anxiety. Siegman and Pope (1965a) manipulated anxiety-arousing properties of topics in the initial interview and observed changes in NonAh rate, although previous factor analysis of speech and other measures had shown negligible loadings of NonAh upon a stress factor (characterised by the Eron Emotional Tone Rating Scale, which may not be appropriate for the experimental situation used) and one which combined GSR with negative affect (again obtained from the Eron scale). However, different intercorrelation matrices of verbal fluency measures show remarkable inconsistency, so much importance should not be placed upon this result. Feldstein et al. (1963) reported NonAh rate higher when patients were discussing their psychiatric problems than other topics.

Several experiments have yielded confusing results. Siegman and Pope (1966a) found an increase in NonAh rate, from low, through medium-, to high-ambiguity TAT cards, which they interpret as having something to do with ambiguous cards' anxiety-arousing properties, though a similar finding (1965a) was not accompanied by a correlation between Eron scale ratings of anxiety and NonAh rate, adding support to the contention that the scale is not very useful in such experiments.

Pope et al. (1970) attempted to manipulate anxiety separate from topic, which had been used in their earlier more successful experiments, by repeating the same interviews twice with thirty two nursing students, informing an experimental group that tests had shown them to be free of psychiatric troubles and feeding the opposite information to an experimental group, before commencing repeat interviews on the pretext that the first set of protocols had been lost. The results are unclear, the NonAh rate dropping, from the first to the second trial, for controls, but not for experimental subjects, when it had been expected to rise for the latter and remain constant for controls. However, the effectiveness of the anxiety manipulation in this experiment is questionable. Mclean (1969) found that an interviewer's either fixed or averted gaze led to an increase in NonAh rate, but averted gaze more. He hypothesised that averted gaze would raise anxiety, which is reasonable in view of three things: its cue-value as an indicator of non-attention and both popular superstition and known facts about trustworthiness and eye-contact avoidance. Fixed gaze had more effect on filled pause rate, but by conveying to the subject either that the interviewer was waiting upon his every word or was excessively interested in his person, this too could have caused anxiety. Reynolds and Paivio (1968) failed to find an effect of the presence of an audience on the NonAh rate of forty eight adult subjects. However, anxiety was not manipulated directly, if at all, in this experiment, and the authors note this.

Two of the best experiments in this area have manipulated both transient and permanent anxiety factorially. Kasl and Mahl (1965) used twentyfive experimental and twenty control subjects, who completed the MMPI and Taylor's MAS and were interviewed, both in the presence

and absence of the experimenter, in a room with one-way observation facilities and with rather doubtful palmar moisture readings taken at intervals. No direct correlation of NonAh with palmar moisture was found, but an interview about stressful topics caused a highly significant rise in the NonAh rate. Manifest Anxiety did not relate to NonAh rate, apart from the merest curvilinear relationship. Cook (1969) used the MAS and another putative scale of anxiety, the MacReynolds Assimilation Scale, which concerns worrying physical symptoms, in contrast to the MAS which asks which topics cause worry (the two scales were found to correlate +.10). Neither scale correlated with NonAh speech disruptions, but transient anxiety, manipulated by topic in Cook's interviews, did increase NonAhs. Cook notes in his Introduction that most experiments which have failed to show a relation between transient anxiety and NonAh disruptions (e.g. that of Pope et al., 1970) have manipulated anxiety ineffectively; the present writer's conclusion is similar. Cook strangely mentions, as demonstrating an association of NonAh rate with situational anxiety, an experiment by Pope and Siegman (1962), but in fact this is not so, since they neither manipulated nor measured anxiety, but assumed the overall speech disturbance ratio (as used by Mahl, 1956) to be an indicator of anxiety, when discussing a correlation between it and ratings of patient anxiety.

2.1.2.2 Dispositional Anxiety. The number of experiments in which anxiety as a permanent characteristic of individuals has been studied in relation to NonAh rate is not so large as for transient anxiety, and the results bear out the lack of relationship found by Kasl and Mahl (1965) and Cook (1969). A team of researchers at McGill University has studied

speakers' audience-anxiety in relation to speech vectors (Levin et al. 1960; Paivio, 1965; Reynolds and Paivio, 1968) and their results do not show a direct link between Audience-Anxiety and NonAhs. In the Levin et al. experiment, fortyeight children aged ten to twelve, of varied Audience-Sensitivity and Exhibitionism (authors' own scale) spoke privately or to an audience. The situation did not have a direct effect upon the global speech-disruption measure they used, but an interaction was significant, most speech errors being made by children high on both audience anxiety and exhibitionism. The Reynolds and Paivio experiment referred to earlier did not show a significant effect of audience-anxiety upon NonAhs. Discussing their results, Reynolds and Paivio note that they may not have affected anxiety by their audience/no audience manipulations and they also suggest something which sounds tautological, that the size of an audience may be salient only to subjects who are high on audience-sensitivity.

Another investigation at the Maryland laboratory (Pope et al., 1970) involved a manipulation intermediate between the transient and permanent anxieties hitherto described, but unfortunately only six subjects were involved, and these were a very mixed bag of psychosomatic in-patients, although the number of different speech samples analysed was ninety six. Each patient was observed daily by nursing staff, for indications of anxiety and depression, and each made a daily ten minute recorded free-speech monologue. The anxiety and depression judgements were used to cast the monologues into a two-factor design and both factors yielded significant results, with several speech measures as dependent variables. NonAh disruptions were high on days patients were judged highly anxious, but bore no relation

to the depression factor.

2.1.2.3 Conclusions. Attempting to put a general interpretation upon all these results, one is bound to note that the relation of NonAhs to anxiety seems to decline with two things: permanence of anxiety and irrelevance of anxiety to the speech itself. When anxiety has been manipulated via the topic of talk relationships have usually been found, otherwise not. The Pope et al. (1970) paper may be an exception to this generalisation, but it is hard to know just where it belongs, since the anxiety involved is semi-transient and since the content of the recorded monologues, in the nature of things, is more than likely to have been to do with the patients' symptoms (particularly so, since they were psychsomatic patients) and therefore the sources of their anxiety. If the previous general statement is true, it seems reasonable to suppose that the effect of anxiety upon speech disruptions is indirect and cognitively mediated by verbal planning; that when discussing anxiety-arousing matters the speaker stutters, hesitates and corrects himself in order to be more precise about what he says, or for concealment. Thus, anxiety's effect upon speech would not be explained in terms of, e.g. anxiety being a drive which 'energizes' speech, causing the speaker to talk too fast and stumble over his words, since, if this were so, the method of inducing transient anxiety, by topic or otherwise, would be irrelevant to the influence of anxiety of NonAhs. Cook (1969) drew a similar conclusion from data on speech rate in his (1969) experiment.

2.2 PREVIOUS RESEARCH ON THE FILLED PAUSE

As stated earlier, the term Filled Pause is used to denote a group of variants of 'Um', which it is important to remember are

perceived in quite a categorical fashion and are easily distinguished from a host of other, involuntary, noises. Intuitively, the speaker-hearer is aware that they, although sometimes irritating to the ear, possess a semi-deliberate quality and are consciously used by some speakers for effect, as the NonAh speech disruptions are not. Unlike NonAhs, filled pauses do not sound like errors, at all. The aim of this section is to elucidate through empirical findings this subjective interpretation.

It needs to be stated about research involving filled pauses that the bulk of experiments has been concerned primarily with other matters and that the data on filled pauses themselves have often been gathered as a peripheral aspect. Many of these experiments have been mentioned in the previous section.

Two hypotheses have attracted the attention of those who study the filled pause: that people utter filled pauses when under stress (the Anxiety hypthesis) and that they utter filled pauses as regulatory cues of the sort outlined in section 1.2, to maintain the floor in conversation (the Floor control hypothesis). These will be dealt with separately.

2.2.1 The Anxiety Hypothesis

If one asks people unfamiliar with the research 'Why do you think people say "um"?' one usually receives the reply that it is because they are nervous. This is altogether reasonable, in view of the connection, fairly well established as the previous section shows, between NonAh speech disruptions and anxiety, about which humanity has had an accurate, if scientifically unproven, intuition for perhaps as

as long as men have been speaking, given the assumption that filled pauses are speech disruptions in the same sense. That is what the earlier investigators of the filled pause assumed, although the evidence is now impossible to reconcile with this anxiety hypothesis. In fact, of Mahl's (1956) eight categories of speech disruptions, all except filled pauses (in Mahl's terms, 'Ahs')and Intruding Incoherent Sounds are defined in terms which make it clear that they are verbal errors. The Intruding Incoherent Sounds form a category of unclassifiable noises, of no relevance to anything, but the filled pauses make an identifiable set of sounds extraneous to the verbal message itself, though not necessarily errors. Whereas stutters, repetitions and incomplete sentences involve breaking linguistic rules, filled pauses are not constrained by linguistic rules any more than the speaker's body movements; they accompany the spoken message without being part of it. Thus, whether or not Mahl at the time imagined that his Ahs would be functionally distinct from the other disruptions he studied, with hindsight it is possible to see reasons for suspecting so. Mahl, of course, considered that any of his eight speech disruptions could prove functionally distinct from all or any of the other seven, but there is one puzzling aspect of his definition of the Ah category which both indicates that he may have thought it special and leaves an unresolved ambiguity in his work. Mahl's definition reads:

> *'Ah.' Whenever the definite 'ah' sound (as distinguished from 'er', 'um', etc.) occurs, it is scored.*
> (1956, p. 2; present author's emphatics)

Subsequently, Mahl specifically included the 'ers' and 'ums' which the above definition clearly excludes, though he does not admit it. In 1959 he quoted the 1956 definition, but omitted the words in

brackets; still later:

> *'Ah.' Wherever the ah sound occurs it is scored. Less frequent variants are 'eh,' 'uh,' 'uhm'.*
> (Kasl and Mahl, 1965, p. 426; present author's emphatics)

The wording of Mahl's original definition is such as to imply Ah to be immediately recognisable, perhaps even a convention, but distinct from the miscellaneous group of unclassifiable sounds, whilst not being, as previously indicated, a linguistic error. The significance of the inconsistency between the earlier and later definitions is probably slight, but a residue of unclarity remains over how results from different experiments are to be understood; did Mahl change his criterion or only the words he used to describe it?

Mahl (1956) reported that an overall ratio of speech disruptions to words, which included filled pauses, varied between phases of patients interviews, with the judged anxiety of the phases. His sample of data was small and unrepresentative, even of clinical interviews, but he later refined his analysis and reported (1959) that Ah did not correlate with judged anxiety in the way that the NonAhs did (cf. 2.1.2). In fact, the evidence now quite rules out any consistent correlation of filled pause rate with anxiety, even though the rate of filled pauses tends to correlate positively with that of NonAhs (of fifteen reported rs the author consulted, all but two were in the range +.2 to +.8). As with the NonAh hesitations, research on the anxiety hypothesis will be reviewed separately for situational and dispositional anxiety.

2.2.1.1 Situational Anxiety. Mahl's 1956 and 1959 material is concerned with situational anxiety and, as just stated, did not

support the anxiety hypothesis. His other main report on the subject (Kasl and Mahl, 1965), which has already been described as rigorous again failed to show a link between either situational or dispositional anxiety and filled pauses. Krause and Pilisuk (1961), who like Kasl and Mahl did find an increase with anxiety of NonAhs under anxious conditions, did not find one for filled pauses, nor did Paivio (1965), who used the presence of an audience as an anxiety-producing treatment.

The Siegman and Pope team has included filled pauses in most of its analyses of speech and has consistently failed to detect a relation between situational anxiety, manipulated by topic and TAT cards (in which latter cases anxiety was monitored with GSR recordings), and filled pauses, although NonAhs have varied with anxiety (Pope and Siegman, 1964; Siegman and Pope, 1965a, 1965c; Pope et al. 1970), and a significant interaction of question-specificity with anxiety occurred in one experiment (Siegman and Pope 1965a). When discussing anxiety-arousing topics, subjects showed a more significantly lower filled pause rate to specific questions than to non-specific questions than when the topic was not anxiety-evoking.

Three faulty experiments have falsely appeared to reveal a link between transient anxiety and filled pauses. Boomer and Dittman (1964) required their subjects to speak under normal, 'speed' and self-monitoring conditions (trying not to say words containing the letter L) and they found an increase which they interpret as having to do with anxiety in the filled pause rate under the last condition, as well as a decrease under the speed condition. They give no details of significance tests, but maintain that the decrease to 0.047 filled pauses per word for the Speed condition, from a Normal baseline of

0.067, is significant at a lower level of probability than the increase to 0.145 for Self-monitoring, which appears unlikely, if parametric statistical assumptions obtained. It is possible that a misprint occurred, reversing the significance levels, but their argument that Self-monitoring is analagous to anxiety is hard to warrant. Moreover, Boomer and Dittman included repetitions as filled pauses, which was later shown to be unwise by Tannenbaum et al. (1965).

Goldman-Eisler (1961a) compared filled and silent pauses, using her standard cartoon tasks and found a highly significant interaction between subjects and cartoons, which she interpreted, without referring to the content of specific cartoons, as suggestive of an emotional factor. Such an interpretation, in view of the actual content of the cartoons Goldman-Eisler used in her experiments, which were unlikely to arouse emotional feelings, is unjustified. More will be said about faults in this experiment under 2.2.2.2.

Panek and Martin (1959) interviewed four psychotherapy patients and analysed for any relation between speech disruptions and GSR, during successive thirty second intervals enclosing dips in the GSR trace. They found peaks in the Ahs (which in their scoring included repetitions) per unit speaking time, coinciding with GSR dips and concluded that both measures validated one another as ongoing indices of anxiety in therapy. It is, of course, impossible for both measures to validate each other, since it would be possible for both to be measuring something other than what is intended, but GSR is already fairly established as an anxiety symptom, so, if there were no other faults in the work the GSR could probably be said to validate the filled pause as an anxiety cue. However, GSR could have dipped because of the

speech disruptions themselves and even though more than sixty observations per subject were recorded, the small number of subjects involved does not warrant much reliance on these results.

Only one experiment known to the author possesses no obvious faults and reports an effect of anxiety upon filled pause rate. Feldstein et al. (1963) varied presence or absence of an experimenter, whether the experimenter spoke or left the subject to sustain a monologue and whether the topic concerned the subject's psychiatric problems. A four-stage interview was conducted with forty eight psychiatric patients of each sex. For male subjects, the filled pause rate was higher in the interview-stages on psychiatric problems than during stages in which autobiographical material was spoken about. Even in this research, however, the effect did not occur with female subjects and the anxiety-evoking properties of talk about psychiatric problems are not fully assured; such talk might alleviate patients' anxiety.

The experiment by Mclean (1969), previously referred to, is peripherally relevant. It showed an increase in filled pauses from a normal baseline whether the interviewer fixed or averted his gaze, but more so for fixed than averted gaze. It was averted gaze which had most effect upon NonAhs and might therefore be interpreted as raising anxiety more, and the results therefore shed little light upon the role of anxiety re filled pauses. Besides, the data are more easily reconciled with the floor control hypothesis to be dealt with below, in 2.2.2. The bulk of evidence clearly does not favour the hypothesis that situational anxiety affects the emission of filled pauses.

2.2.1.2. Dispositional Anxiety. Five studies have been traced which bear directly upon dispositional anxiety and filled pause emission (Kasl and Mahl, 1965; Siegman and Pope, 1965b; Cook, 1969; Paivio, 1965; Reynolds and Paivio, 1968), but, of these, only Siegman and Pope's reported significant association, and that negative, using a shortened Taylor Manifest Anxiety Scale ($r = -.24$; $n = 50$).

There have also been cases of minor relevance to the issue, which have studied intercorrelations between speech vectors or have used measures of marginal relevance to anxiety. The factor analysis of speech measures GSR and the two Eron affect ratings by Pope and Siegman (1964) did not show a significant loading for filled pauses on the supposed affective factors, but, although the study was of individual differences, affect readings were mainly transient. Filled pauses correlated insignificantly with all other measures in the Pope et al. (1970) work, which was again mainly on transient manipulations, though permanent individual differences would contribute to any intercorrelations. Levin and Silverman (1965) interpreted one of four factors they obtained from a sample of children's speech, and on which filled pause rate (which may have been contaminated by momentary glottal catches) loaded, as a stress factor. As in the two foregoing experiments, manipulations were transient, but it is in any case hard to accept the authors' interpretation of this factor, on which silent and filled pauses loaded negatively and sentence corrections positively, since there is no evidence that either silent pauses or sentence corrections indicates anxiety, except that in most work the latter measure would be included among NonAhs. However, other NonAhs were scored and did not load on the factor in question, which is more likely to be cognitive, if anything.

The study by Pope et al. (1970), in which nurses judged patients' daily levels of anxiety offers no support for a link between longer-term anxiety and filled pause rate -- even if it were possible. Mahl (1958) mentioned that high emitters of filled pauses tended to have histories of early weaning, parental strictness and obsessiveness, but the relation of these to anxiety is complicated. Evidence on the whole strongly suggests that there is no more connection between dispositional than transient anxiety and uttering 'ums' and 'ahs'.

2.2.2 The Floor Apportionment Hypothesis

Maclay and Osgood (1959), after noting what appeared to be a weak complementary distribution between filled and silent pauses in the speech sample they studied, with filled pauses occurring more at phrase boundaries than silent pauses, offered an hypothesis to explain this.

> *What about the distinction between Filled Pause and Unfilled Pause? We suggest that the main distinction lies in the duration of the non-speech interval. Let us assume that the speaker is motivated to keep control of the conversational 'ball' until he has achieved some sense of completion. He has learned that unfilled intervals of sufficient length are the points at which he has usually lost this control - someone else has leapt into his gap. Therefore, if he pauses long enough to receive the cue of his own silence, he will produce some kind of signal ([m,ər], or perhaps a repetition of the immediately preceding unit) which says, in effect, 'I'm still in control -- don't interrupt me!' We would thus expect Filled Pauses and Repeats to occur just before points of highest uncertainty, points where choices are most difficult and complicated. We have also noted that Filled Pauses tend to occur at the junctures of larger syntactical units, presumably where constructional decisions as to what to say, content-wise, are being made. This assumption that 'ah'-type pauses are reactions of the speaker to his own prolonged silences at points of difficult decision is consistent with our finding that these two pause-types are merely statistically, not absolutely, different in distribution. A difficult choice can occur almost anywhere, although it is more likely at points where both structural and content alternatives are being juggled.* (p. 41)

This passage sets out an hypothesis that filled pauses function to enable the speaker to hold the floor in conversation and it is this hypothesis that the present research was designed to test. However, in adumbration of later discussion, two points may be made about the Maclay-Osgood formulation. The first is that it is implied, albeit weakly, that conversation involves competition for the floor, which may not be the case and which anyway need not be so for a version of the floor control hypothesis to be tenable. A speaker might inform his interlocutor that he had not finished his utterance, in order to, e.g. spare him effort (in trying to find something to say) and embarrassment, when a pause occurs, or possibly to summon his wandering attention. Indeed, one might well preface remarks with a filled pause in order to signal that one has accepted the floor, when it has been offered. Thus, floor control may be conceived in a broader regulatory sense than that of the Maclay and Osgood paper. The second point is that the authors suggest the speaker may respond to his own silence, when, again, this is not essential to the hypothesis that filled pauses are floor control devices. The possibility may not be discounted that a speaker anticipates pauses of excessive length and interpolates filled pauses as soon as he reaches them.

Research relevant to the floor control hypothesis may be divided into four types: studies of the linguistic, task, situational and personal contexts.

2.2.2.1 Evidence from the distribution of the filled pause. The first important study in this category has already been partly described, viz. that of Maclay and Osgood (1959). Those authors observed that filled

pauses were relatively more likely than silent pauses to occur before functors as opposed to contentives ($p < .001$), as shown in Table 2.1.

TABLE 2.1

DISTRIBUTION OF FILLED AND SILENT PAUSES BY FOLLOWING WORD CLASS (after Maclay and Osgood, 1959)

	Filled Pauses	Silent Pauses	Total
Lexical Words (Contentives)	418	525	943
Function Words (Functors)	360	290	650
Total	778	815	1593

In addition, they presented figures for the occurrence of filled and silent pauses at different positions in particular types of phrases, which show that filled pauses occur relatively more at phrase boundaries (i.e. syntactical junctures) than silent pauses. In Table 2.2, some illustrative figures from Maclay and Osgood are shown, with frequencies of filled and silent pauses at different numbered positions in three of Fries' phrase types, with the associated Chisquare probabilities indicated.

TABLE 2.2

DISTRIBUTION OF FILLED AND SILENT PAUSES BY POSITIONS IN SELECTED PHRASE TYPES, WITH ASSOCIATED CHISQUARE PROBABILITIES (after Maclay and Osgood, 1959)

Fries' Sequence Type	Example		Positions 1	2	3	4	5	Chi-square P
A31	1the2red3houses4	FP	24	24	19	35		.01
		SP	17	36	35	19		
F31	1across2wide3streets4	EP	11	35	12	22		.05
		SP	7	18	13	6		
FA31	1under2the3big4house5	FP	14	8	13	11	16	.001
		SP	14	16	32	17	7	

Cook (1971b) has pointed to defects in the Maclay and Osgood report, namely that no account was taken of the slightly different baselines for functors and contentives, that they selected for analysis a sample of speech supposedly made up of representative grammatical sequences and that this sample included no utterances shorter than eighty words in length. Cook himself had shown (1968) that longer utterances have lower NonAh, though not Ah rates. In his later paper, he presented his own results, based upon half-hour interviews with eleven speakers, in which was no evidence that filled pauses are relatively more likely to occur before functors than contentives. Within the overall categories of functional and lexical words, however, there is some divergence of patterns between individual word-classes, although inter-subject differences of patterns are negligible, with only one, minor, exception, in the case of conjunctions. Although the Maclay-Osgood results appear to be partly artefactual, Cook mentions that his own are not inconsistent with the floor control hypothesis,

since filled pauses tended to occur, as would be expected, at the beginnings of clauses. He wrongly imputes to Maclay and Osgood the assumption that uttering speech is no more than word-selection and there are some small arithmetical irregularities in his own figures, as presented, possibly printers' errors.

In contrast to Cook's findings, Blankenship and Kay (1964) found that in a sample of five hours speech to audiences, which, like that of Maclay and Osgood, must have contained only long utterances as only ten speeches were involved, filled pauses tended to precede functors, rather than contentives. However, even Maclay and Osgood only reported such an effect relative to silent pauses, with both types of pauses predominantly happening before contentives, whereas Blankenship and Kay show only thirty seven per cent of filled pauses before contentives. Like Maclay and Osgood, these authors do not take basal rates of functors and contentives into account, which could possibly explain the anomaly.

Boomer's (1965) well-known paper previously referred to in 2.1.1, interpreting his data in the terms suggested by Trager and Smith (1951) of phonemic clauses, showed filled pauses more than silent pauses tend to occur before, rather than after, the first words of phonemic clauses. Hawkins (1971), used syntactic, not suprasegmental, criteria and did not distinguish filled from silent pauses, in an attempt to check some of Boomer's findings. He succeeded in showing that pauses happen at the beginnings of clauses, but, his data throws little light upon either silent or filled pauses alone. Boomer's report also observes that Ahs were not often preceded by unfilled silences and suggests that they are therefore not responses to the

speaker's own silence. It has already been argued that such an eventuality does not discredit the floor control hypothesis, because a speaker may respond to anticipated silence.

Two studies have presented statistical predictabilities for words following filled pauses (Tannenbaum et al., 1965; Mishler and Waxler, 1970) and both reported that words of low predictability follow filled pauses. The Mishler and Waxler work involved both normal and schizophrenic speech, but there is nothing abnormal about schizophrenic speech, as such (Brown, 1973), and if there were the consistency of the results would enhance the generality of their import. Tannenbaum's data displayed low predictability of words subsequent to filled pauses on four different measures out of five, the only reversal being for predictability of form class. Another finding, which casts doubt upon the previously mentioned results of Boomer and Dittman (1964), is that repetitions, one of Mahl's NonAh categories, which have sometimes been assumed, and which were suggested by Maclay and Osgood, to be functionally identical with Ahs, showed an increase in predictability from preceding to subsequent words, the opposite of the finding for filled pauses. This was so on four of Tannenbaum's five predictability measures, the reversal being statistically insignificant. Although it would be unwise to assume that speech is uttered in individual words, rather than larger linguistic units, the predictability data for filled pauses lend support to the notion that they happen at choice points where silent hesitations might be likely or prolonged.

2.2.2.2 The influence of the task. Goldman-Eisler (1961a), comparing filled and silent pauses, claimed to find no evidence that the former, like the latter, increased with difficulty of the verbal task, i.e. from descriptions to interpretations of cartoons. Paivio (1965), however did find such an effect, and reports confirmation of this by his pupil, Lay (1964). Their experiments apparently did not suffer from the faults of Goldman-Eisler's. Siegman and Pope (1965a, 1966 a & b) have consistently found a higher filled pause rate in verbal responses to ambiguous than to specific stimuli, although the same authors report one failure to obtain such an effect (Siegman and Pope, 1968) and do not report any analysis for this in another paper (Pope et al., 1971) in which interviewer specificity was varied between questions, upon twenty four psychiatric patients. It is Siegman and Pope (1966a) who criticise Goldman-Eisler for her idiosyncratic filled pause measure, which expresses filled pauses as a percentage of total pause time, instead of the more usual ratio to number of words. They draw to attention that on a different page from her main consideration of the difference between descriptions and interpretations, she presents figures, more akin to the conventional measure, which are as one would expect, i.e. with a higher filled pause rate per word for interpretations than for descriptions (Goldman-Eisler, 1961a, p. 21). A sign test on the figures yields a probability of 0.062, but a related samples t-test falls shorter of significance ($p < .10$), apparently because of the effect upon variance of a subject ('An', in Goldman-Eisler's paper), whose scores, although showing a colossal difference in the appropriate direction, were far outside the range for the other subjects. Dropping this subject from the test, $t = 1.989$, with 7 df; $p < .05$ (2-tailed test), so

Goldman-Eisler's evidence is by no means strongly counter to those of Paivio and the Maryland team. The present writer would also criticise Goldman-Eisler's paper for the way in which, in different parts of the report where figures for individual subjects are presented, subjects come and go with no reasons given. A total of twelve different speakers is identified by abbreviations, yet the number of speakers actually used in any one statistical test reported in the paper varies between four and nine, as far as the reader can tell, with no clear explanation.

In connection with task difficulty, in the paper which was criticised earlier, Boomer and Dittman (1964) observed a high filled pause rate when subjects were self-monitoring. The self-monitoring task would be an extremely good example of one in which the verbal task itself is cognitively difficult.

Cook (1971b) has stated that two factors are really involved in the floor control hypothesis: that cognitive difficulty leads to hesitant speech and that hesitation, under risk of interruption, leads to filling of pauses. Hence, cognitive difficulty should lead to a high rate of filled pauses, which is what the immediately foregoing paragraphs despite Goldman-Eisler's interpretation of her results seem to confirm. There is ample evidence (cf. 2.1.1) that the first factor of the hypothesis has been demonstrated to hold. Attention will now be turned to results more relevant to the second factor, proper, which is already partly supported by the evidence on cognitive difficulty and the incidence of filled pauses.

2.2.2.3 The influence of the situation. If the floor control hypothesis holds good, two types of situational manipulations should

increase the incidence of filled pauses: increasing risk of interruption and interfering with alternative, established means of apportioning the floor. Only one attempt to test the former possibility, prior to Experiment II of the present work, has been traced, that of Lalljee and Cook (1969).

On the basis of a short preliminary interview, Lalljee and Cook matched for Filled Pause rate and verbal productivity a total of seven pairs of subjects, who subsequently participated in individual discussions with a secret confederate about topics of mutual interest on which they disagreed. One group of subjects received from the experimenter a casual remark that the other person was 'a very fluent speaker, so you might have difficulty in getting a word in. Nevertheless, we would like you to do your best to get a fair share of of the conversation, and if possible to change his view,' and were interrupted by the confederate, according to a prearranged schedule. The confederate interrupted at the beginnings of the discussions, thereafter starting to speak whenever the subject stopped or paused. The other group of subjects received no special instructions and the confederate took care to let them finish what they had to say before speaking himself. The last five minutes of the ten-minute conversations were analysed. The attempted induction of pressure to avoid interruption was validated by significantly greater verbal output and more interruptions of the confederate by the subject under the first condition than the second, although the former measure is only reported for both speakers combined and could therefore be affected by the confederate alone. NonAhs were not significantly different for the two groups, so anxiety was probably not affected by the manipulation. Control subjects had non-significantly higher mean rates for both filled pauses and repetitions than the experimental subjects.

In discussing their results, Lalljee and Cook conclude that the Maclay and Osgood theory is not correct, at least for the dialogue situation used in their own experiment. They suggest that it may apply to monologues and quote the mean filled pause rates for their experiment proper (0.0308) and for the non-directive interview, used to match subjects which they assert was 'essentially a monologue by the subject' (0.0389), as well as similar figures from an unpublished experiment, which show a similar, but this time significant, difference.

In criticism of the paper by Lalljee and Cook, three points arise. Although they managed to show that their manipulations had a significant effect upon the subjects' interruption-rates and the combined verbal productivity of subject and confederate, these two measures would be expected to be affected more directly by the manipulations than the NonAh and filled pause measures, which, with only seven pairs of subjects might well not display the effect sought. The second criticism is that the induction procedure, with its warning about the 'fluency' of the confederate, might have caused suspicion on the part of subjects. This is reinforced by the fact that Cook (personal communication) has described his confederate as 'very enthusiastic'. Finally, floor control by nature can really only occur in dialogues, which makes the suggestion that the Maclay and Osgood theory may apply only to monologues intrisically absurd. It is true that the latter authors used material from a scientific conference, but their description of how they selected utterances for inclusion suggests that discussion with at least some prospect of interruption, rather than formally presented papers, was involved. In any case, whereas it is easy to imagine that a habit of filling pauses, acquired in dialogues might generalise to monologues, such a habit would hardly be learned

in the latter. Moreover, although it is impossible to comment on the unpublished experiment referred to by Lalljee and Cook, they effectively overstate the difference between pre-interview and experiment proper in their published paper, since they quote the combined mean for their two subject groups, when one of these had received a bizarre treatment. The mean for the control subjects, at 0.03359, is closer to that for the pretest and because of the matchings, should be a good estimate of the mean for both groups in 'normal' dialogue. Experiment II, of the present series, was carried out to retest the same prediction as that of Lalljee and Cook, and, as will be seen, produced similar results.

Three experiments have involved interference with visual means of floor control, as does Experiment I of the present work. Kasl and Mahl (1965) have already been referred to in other connections. Interviews were conducted in an observation room with a one-way mirror. Subjects wore headphones throughout, which enabled part of each subjects' first interview to be staged with the interviewer present and visible and part with the interviewer out of sight, though, unbeknown to the subject, watching him from the observation room. Filled pause rate was higher under the latter condition, which is consistent with what would be expected if the filled pause is a device for holding the floor. With such visual methods as gaze direction ruled out, the speaker would have to turn to vocal cues like filled pauses. Although the Kasl and Mahl experiment was well-conducted, more evidence is needed, in view of the artificiality of communicating with an interviewer by head-set. Even if this was only replaced by another artificial condition, it would broaden the basis of the evidence, which is what Experiment I was meant to do. However, Mahl (1958) reports briefly that filled pause rate was higher for telephone conversations than in face-to-face

interaction, and it must be agreed that the telephone provides a highly appropriate form of oral communication under visual restrictions for the purpose required. More recently, Siegman and Pope (1968) have reported a rise in filled pause rate when subjects were interviewed with a screen cutting off visual communication. This effect was significant at the .01 level, although its magnitude was only of the order of 20%.

Lalljee and Cook (1969) refer to an unpublished experiment by themselves which confirms these findings on visibility, but the author has been unable to obtain any details from Dr. Cook, other than that only two levels of the visibility variable were employed and that the analysis was uncompleted at the time (1971). It appears not subsequently to have appeared in print. Mclean (1969) observed more filled pauses when an interviewer either gazed continuously or averted his gaze than under normal interviewing conditions. The eyes function in encounter regulation (see 1.2.2) both in terms of where one is looking and in terms of the timing of eye movements, so cutting out eye movements altogether, by either of Mclean's methods would rob gaze direction of all cue value and probably be capable of increasing filled pause emission. Only a post-hoc explanation can be offered for the fact that Mclean found a higher filled pause rate for fixed than for averted gaze, but it could be that the gaze implied to the subject that the interviewer was anxious to know what else, if anything, he had to say. On the other hand, it could have been argued that the implied loss of the attention of a gaze-averting interviewer, would be more likely to raise a subject's filled pause rate. Mclean (personal communication) in fact averted his gaze downwards, which could be relevant. Libby's work (1970, 1971) on direction of 'Look-away' does

not tell us enough to be certain on this point, but one might hazard the guess that a gaze-averting interviewer would be more likely to convey to the interviewee the impression of inattention if he appeared to be thinking about something else than if he buried his eyes in his chest, which suggests, says Libby, submission. Clearly, some interesting experiments remain to be performed in this area.

Lalljee and Cook (1969), as mentioned, observed higher filled pause rates in non-directive pre-interviews than in actual experimental sessions, and suggest that the former approximate to monologues and consider that monologues generally have higher filled pause rates than dialogues. More direct evidence suggests otherwise and, insofar as it could, provides support for the Maclay-Osgood hypothesis. Feldstein et al. (1963) used both monologues and dialogues and found more filled pauses in the latter. Siegman and Pope (1966a) noted less filled pauses in their TAT experiment (monologues) than in a previous one (1965a), which used interviews. Attempting to compare filled pause rates from separate experiments by different authors and carried out under widely different conditions, to throw light upon the monologue-dialogue issue is not recommended and an attempt by the writer to do this proved fruitless, owing to the variations in conditions used. Taking the figures of Feldstein et al. (1963) and Siegman and Pope as the best available, it does appear that dialogues, in which interruption is possible, elicit more filled pauses than monologues, in which the floor is guaranteed.

2.2.2.4 The personal context of the filled pause. Not much is known about the kinds of people who emit few or many filled pauses, but data from most experiments have shown large individual differences. Siegman and Pope (1965c, 1966b) obtained correlations of -.32 (n = 50)

and -.55 (n = 16) between filled pause rate and the Extraversion scale of the Maudsley Personality Inventory. This is very much what would be expected, on the basis of the floor control hypothesis, in view of the Ramsay (1966, 1968) findings on silent hesitations and extraversion : introverts pause more than extraverts and therefore have more pauses to fill. Moreover, Eysenck's theory of introversion-extraversion, which stresses an underlying inherited tendency towards cortical excitation or inhibition, maintains (Eysenck, 1957) that introverts are over-socialised and extraverts under-socialised, where social norms and mores are concerned. The floor-apportionment hypothesis treats filled pauses as social cues used to lubricate the machinery of encounters and it would only be expected, therefore for introverts to emit more filled pauses, since their social overlearning would make them more sensitive to regulatory requirements than extraverts.

The work of Paivio and his McGill associates on speech, exhibitionism and audience anxiety has not yielded much of interest on filled pauses, much of the work not separating them from disfluencies. Paivio (1965) however, reports that Lay (1964) found more Ahs emitted by subjects low on exhibitionism than by exhibitionistic subjects. It seems most likely that this difference is just the extraversion-introversion effect under another name.

Mahl (1956) stated that individual differences in filled pause emission correlated with early weaning, strict parental control and obsessive habits, but he has not enlarged on these statements in later papers.

Feldstein et al. (1963) report a higher filled pause rate for males than for females and a positive correlation (r = .296) between filled pause rate and amount of education, for their psychiatric outpatient sample forty eight of each sex. Reynolds and Pylyshyn (1970) found no differences in emission of filled pauses between several groups of psychiatric patients and this may add to the confidence with which the Feldstein results may be extrapolated to wider samples of subjects.

Taken together, the results on individual differences add little to what has previously been reviewed. Differences between introverts and extraverts can be easily reconciled with the floor control hypothesis, but well-established emotional correlates of filled pauses are absent.

2.2.2.5 Three other experiments. Only three experiments have been reported, in which the filled pause was an independent, rather than a dependent, variable. Cook and Lalljee (1970) carried out two similar experiments, one with short recorded sentences and the other with recorded random numbers.

In the sentence experiment, half were grammatically complete and half not, and of each, half terminated with filled pauses and half not. Subjects had to imagine they were conversing with the speaker and press a button when they thought they would reply to each utterance. In the experiment with number lists, some lists had silent, some filled and some no pauses inserted and subjects had to spot the last number in each list before the speaker announced that he had finished. In the first experiment, grammatically incomplete sentences led to longer delays before button-pressing, but the opposite was true for sentences ending

with filled pauses. In the second experiment, silent and filled pauses both caused subjects to think the number lists were finished. The authors concluded that the Maclay-Osgood hypothesis is unsupported by these results. Both the Cook and Lalljee experiments involve unusual situations from the subject's point of view and there is cause to doubt their representing well how we behave in normal practice. The authors report that many subjects (in the first experiment) said that the statements used did not seem to call for an answer at all', which reinforces this point. In the case of the numerical material, it is possible that listeners assumed the speaker to be reading the lists and therefore did not expect any extraneous sounds to occur in mid-list, hence treating the filled pauses as terminal cues. Experiment IV of the present work involves an elementary adaptation of the Cook and Lalljee technique to more natural conditions.

Finally, although less strictly relevant to the floor control hypothesis, an experiment by Livant (1963), like that of Cook and Lalljee, employed the filled pause as an independent variable. The floor control hypothesis, because it implies that filled pauses occur when the speaker has to think, would not suggest that quality of the cognitive product is lower when filled pauses are emitted than when they are not, even if speech becomes a bit inelegant. Goldman-Eisler (1961a) argued that the quality of speech with high filled pause rates was lower than for that with low filled pause rates, but her judgement was subjective and therefore suspect. Livant required five subjects to complete simple arithmetical problems under two conditions: silently and continuously uttering Ahs. He found that every subject took much longer over the problems when vocalising. Apart from small sample size, which is only a trivial issue, in view of the magnitude of the effect,

it is blatantly obvious that subjects had to monitor their behaviour in the continuous vocalisation condition and this would increase cognitive load. An improvement would be to preselect subjects for their normal filled pause emissions when solving problems and determine whether subjects who normally did so would perform worse when forced to solve problems silently. Livant's experiment has been unilluminating.

2.2.3 Conclusions about research on the Filled Pause

Unlike NonAh speech disruptions, Ahs appear to be unaffected by anxiety, but to relate to the speaker's cognitive activities, on all available evidence. Difficulty in accounting for how thinking can cause such meaningless and non-verbal noises to be uttered, makes it strongly tempting to suppose that they function in regulating the social encounter, buying the speaker time for verbal planning. Direct interest in testing the floor control hypothesis of Maclay and Osgood has been less widespread than readiness to include filled pauses in batteries of dependent variables, so that there is quite a lot of relevant but indecisive evidence.

Such evidence as is available appears mostly to be consistent with the floor control hypothesis, although that of the Oxford researchers, Cook and Lalljee, who have been directly interested in the filled pause, provides no positive support. However, their experiments and interpretations are open to criticism on several grounds and do not resolve the issue. It seems well established that filled pauses are emitted more, when speakers have to think about what they are saying and there is suggestive research on their situational correlates. Individual differences found to be related to filled pause rate are either consistent with, or irrelevant to, the floor control hypothesis.

Thus, there is no justification for joining Cook (1971a) in his conclusion: 'In fact, they do not express the speaker's desire to continue speaking, nor are they recognized as such by the hearer. Their function, if any, is still uncertain!' (p. 80). Robinson (1972), stated of the floor retention hypothesis, that: 'Unfortunately this reasonable idea has foundered on the rocks of hard data', and went on to suggest an alternative, related hypothesis, that filled pauses are used to gain the floor. He cited Boomer's (1965) observation that only eight per cent of silent pauses happened before the first words of phonemic clauses, in comparison with twenty five per cent of filled pauses (Robinson wrongly states, twenty two per cent). Robinson actually appears to mistake phonemic clauses for whole utterances, since the beginnings of most phonemic clauses would be in mid-utterance, Boomer having asked subjects 'to speak extempore for about three minutes' on topics of their choice (present author's emphatics). Nonetheless, Robinson's point is well taken; if the floor-control hypothesis narrowly defined, does not hold water, a 'floor-grabbing' hypothesis would be an attractive alternative. Better still would be to define floor control more broadly than merely floor-retention, something which has been implicitly behind much of the present opus, up to this point. It remains anyway to be seen to what extent redefinition of the hypothesis may be necessary, but the research already reviewed would appear to imply that wider aspects of attention in conversation are involved than just retention of the floor, once obtained, or its assumption to begin with.

2.3 THE PRESENT SERIES OF EXPERIMENTS

Because every experiment has its own Introduction, this section does not present the full rationale for each one. Instead, the general nature of the problem to be investigated and methodological considerations not dealt with under individual experiments are outlined.

2.3.1 The Problem

The floor apportionment hypothesis, as advanced by Maclay and Osgood (1959), was conceived in fairly narrow terms. It was implicitly assumed, that a conversation is a competitive encounter with each participant trying to gain as much of the floor as possible. If the results of the Oxford researchers who have attempted to test the hypothesis directly have not supported its, this may in part be because they tested the hypothesis only in its narrow form (e.g. Lalljee and Cook, 1969). Floor apportionment involves more than just avoiding interruptions: extended silences are usually equally undesirable. The present writer starts therefore by stating the hypothesis in fairly general terms: that filled pauses are involved in regulating encounters, functioning *inter alia* to warn that a person wishes to assume the floor, to inform the listener that a speaker does not expect a response from him, to retain the floor or the listener's attention, or combinations of these.

Floor control does not necessarily mean *possession* of the floor, merely control over who shall have the floor, which may be exercised in order to avoid speaking just as much as to speak. In most conversations, where hostility is minimal, participants presumably aim to maximise *joint* control of the floor, i.e., to see that each gets his

chance to say what he wishes and no more, insofar as the individual goals are compatible. Thus, one would expect that interference with normal alternative methods of floor regulation would raise filled pause rate. Again, one might expect that an interlocutor greedy for the floor would, if the subject had anything to say, evoke more filled pauses. Experiments I and II were carried out to test these predictions.

If Ahs do have a regulatory role like the one proposed, it should follow that speech rich in them would lead to particular states and motives being attributed to the speaker. Moreover, it has been found that introverts emit more filled pauses than extraverts and so we might also expect personality traits related to introversion-extraversion to be associated with high filled pause rates. In Experiment III these predictions were tested. Experiment IV used a behavioural measure of subjects' readiness to assume the floor after his interlocutor terminated utterances with or without filled pauses. Such an experiment should provide a fairly direct test of whether filled pauses do convey to the listener information about a speaker's floor-regulatory intentions.

Summing up the goals of the present work, it was carried out to test the Maclay-Osgood hypothesis that Ahs are used to forestall interruption and to throw possible light upon whether a more general form of the hypothesis than the original would be more tenable, using a varied set of manipulations for this purpose.

2.3.2 The Methods

The present project has employed throughout laboratory methods, rather than those of the field. Laboratory experiments are of limited

value, particularly when conducted under conditions unfamiliar to participants and with intrusive physical constraints upon them, but care was taken in the present series, to simulate natural conditions as well as could be done. Experiment III involved subjects in completing booklets of rating scales, but being students of psychology subjects were familiar with this sort of task and would therefore be able to cope with its demands.

In all except Experiment III, direct behavioural measures were employed. Experiment IV used timed latencies for subjects' next remarks after utterances by a confederate and such latencies are very easily measured with stopwatches to more than adequate accuracy. More details about the other measures used are to be found below.

All subjects were naive to the purposes of experiments in which they participated. They were, however, informed about the hypotheses immediately after taking part, and were invited to pass comment.

2.3.2.1 Measurement of the Filled Pause Rate. Except for the papers by Goldman-Eisler (1961a) and Panek and Martin (1959), investigators have expressed frequencies of filled pauses as ratios to the number of words uttered, sometimes multiplying by a large constant to avoid presenting small decimal fractions. This report follows the same practice and filled pause rate will be expressed as Ahs per thousand words.

Fortunately, filled pauses are easy to detect and can be scored with high reliability. There are rare occasions when [Ah] may be confused with [and], but in general it is at least as easy to

detect as a true word. Robinson (1972) states that filled pauses are 'Defined somewhat casually but satisfactorily as "ahs" "ers" and "ums"'and it is perhaps one of the more interesting facts about them that they are so easily recognised, although phonetically diverse and indistinct. Mahl (1956, Kasl and Mahl, 1965) has reported very high inter-rater correlations for Ahs, of the order of .99, based upon the rater's total scores per page of unselected transcripts. Pope and Siegman (1964) refer to their own reliability study, in which they determined the percentage of agreement between the authors over individual instances; the figure was eighty nine per cent, which would yield a very high correlation, if totals over several sentences were to be used, as by Mahl. Because of the obvious high reliability with which filled pauses are identified, the present writer followed normal practice and did not carry out a special reliability study of his own. However, he would point out that the reliability of the filled pause ratio depends upon the reliability with which words can be counted as well as on the reliability of scoring Ahs, though to a lesser extent, since there are typically between ten and a hundred words for every filled pause, which means that unreliability in word-counting is partly offset by the size of the word samples.

2.3.2.2 The Semantic Differential and listeners' ratings. Rating scales are always subject to limitations of which the researcher needs to be aware. They encourage and often compel the subject to discriminate between things he may not normally distinguish and may create thereby false impressions of differences which do not really exist. Stereotypes in the thinking of the subject sample may generate similarly spurious differences. The use of rating scales, with Semantic

Differential format (Osgood et al., 1957) in Experiment III was decided upon with slight reluctance, but convenience militates heavily in favour of such scales in these cases, in comparison with alternative possibilities. Numerical data are obtained quickly and easily and are conveniently subjected to statistical treatment. The development of behavioural measurement techniques which will replace rating scales is really in its infancy and, even if it were not, such techniques would not permit subjects to be run in numbers as large as were possible for the experiment in question.

Adoption of Semantic Differential format and the inclusion of many scales used by Osgood and his colleagues should not be interpreted as implying any committment on the part of the author to the associated theory of Representational Mediation Processes. The theory has been criticised by both linguists and psychologists (Carroll, 1959; Fodor, 1965; Miron, 1969, a & b; Weinreich, 1958) on a number of points and a satisfactory rebuttal has been unforthcoming. Osgood has (1966) attempted to clarify what he means by the r_m and has further elaborated on this (1969a), arguing that the construct is rather like the phoneme or sememe, in linguistics, and that Fodor and others behave as if they confuse individual phones with the phonemes of which they are exemplars. Nonetheless, the symbols and language chosen by Osgood, immediately associated with a whole tradition of S-R psychology, would scarcely be expected to do other than lead readers to the interpretations at which he protests.

Despite the questionable nature of Representational Mediation theory, the value of the results achieved from use of the Semantic Differential and the generality of the finding that the affective

reactions tapped by it resolve themselves into variants of a three dimensional main structure of Evaluation, Potency and Activity, are widely acknowledged, even by Osgood's main critics. To employ the format and make use of data from Osgood and his colleagues in selecting scales for use does not presuppose accepting Osgood's theory which, he is reported (Miron, 1969a) to have admitted, is only loosely related to the practical tool. In Experiment III, the author has used the Semantic Differential, but, if pressed for a theoretical stance on the question of what the scales represent, would prefer to adopt that of Kelly's Psychology of Personal Constructs, for reasons beyond the scope of the present thesis. Personal Construct Theory bears a more than superficial resemblance to Osgood's theory, in certain aspects; the associated practical techniques are exchangeable; but, as Bannister and Mair (1968) point out, the theories themselves are distinct on essential points. It is the present writer's belief that Kelly's theory provides a more convenient terminology for his purposes than Osgood's.

CHAPTER THREE

THE FILLED PAUSE AS A DEPENDENT VARIABLE

3.1 EXPERIMENT I

THE EFFECTS OF VISUAL RESTRICTIONS ON EMISSION OF FILLED PAUSES

Aim. To establish whether restrictions upon the availability of visible cues to floor apportionment increase the incidence of filled pauses.

3.1.1 Introduction

The Oxford theory of social interaction as a quasi-motor skill predicts increased use of alternative channels to transmit social information when one or more other channels is blocked. Since gaze direction (in the visual channel) is already known to be an instrument of floor control, it follows that other, invisible cues should be used more when eye-contact and like visual cues are precluded and this provides a possible test of the hypothesis that filled pauses have a floor apportionment function. The experiments on effects of visual restrictions reported by Moscovici (1967) and Argyle et al. (1968) did not involve monitoring filled pause rate and neither that of Kasl and Mahl (1965) nor that of Siegman and Pope (1968) contained levels of visibility intermediate between full and zero.

In the present experiment, four degrees of mutual visibility were employed in dyadic conversations, following the methods of Argyle et al. (1968): normal visibility; both subjects wearing dark sunglasses; both subjects wearing dark sunglasses and visors which obscured the whole face (cf. Appendix 3.1); a screen placed between the pair, cutting each off completely from the other's view. The last three treatments

progressively cut out movements of the eyes, face, limbs and trunk, as sources of social information. Since a repeated measures experimental design was employed, four topics of judged equivalently neutral nature were selected for conversation: Censorship, Education, The Mass Media and Punishment.

3.1.2 Experimental Design

A repeated measures design was chosen for economy of subjects, although this necessitated control for conversational topic, as pairs of subjects could not be expected to discuss the same issue four times. The solution selected was a 4 x 4 Graeco-Latin Square (cf. Appendix 3.2), although by confounding topics with trials a plain 4 x 4 Latin might have sufficed.

Two pairs of subjects were observed in each cell of the design and it was considered that for all practical purposes two within a pair could be treated as independent emitters of filled pauses, so the analysis assumed the square to have been replicated four times, fitting Winer's Plan 7 (1962, p. 577).

The hypothesis tested in Experiment I was that the rate of emission of filled pauses would be inversely related to the degree to which subjects were visible to each other.

3.1.3 Method

Subjects.

Sixteen female Psychology I students at the Queen's University of Belfast, aged eighteen to twenty four years; all volunteers and unmarried.

Materials.

Audio-tape recorder and microphone, cards bearing instructions and discussion topics, visors, 1.5 x 1.0 metre screen, one table and three upright chairs.

The experimental room, which was curtained and carpeted and normally used for test demonstrations, was set out as shown in Figure 3.1. The microphone was concealed behind a curtain which covered the one-way screen and subjects sat about one and a half metres apart, where they

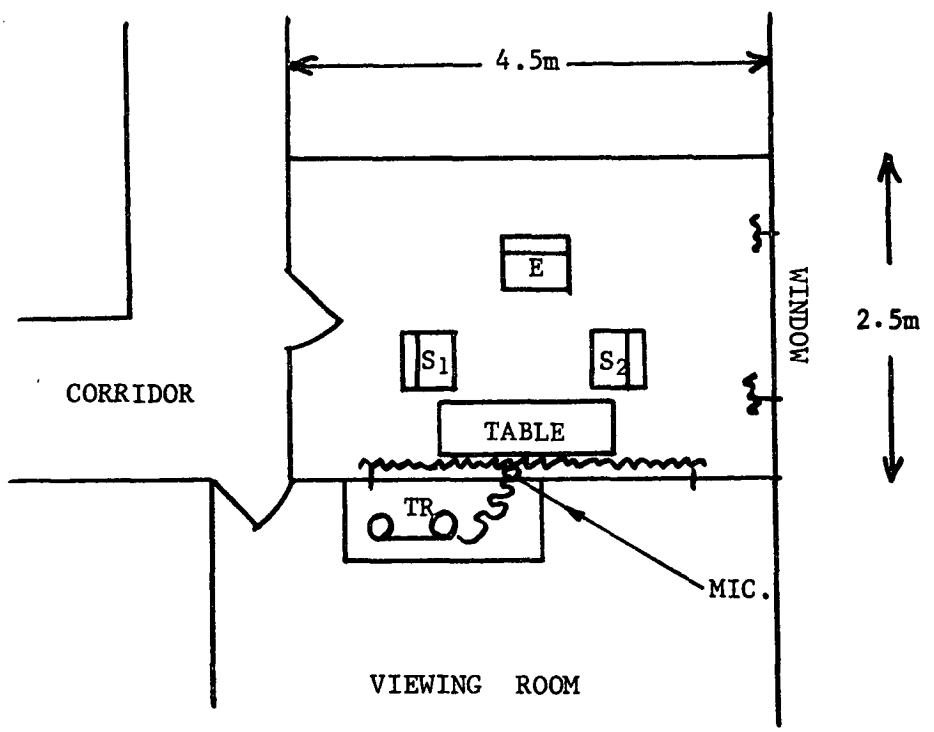

Figure 3.1 Laboratory Plan for Experiment I

could each rest one elbow on the table, upon which instruction cards were placed.

Procedure.

Arrangements were made at laboratory classes for subjects to attend in pairs and the experiments being conducted at the beginning of an academic year ensured that members of any given pair were not already well acquainted.

Subjects were led to their chairs and received instructions (cf. Appendix 3.3) about the general nature of their task. The experimenter pointed out the position of the microphone, explaining that it was concealed to avoid the sight of it inhibiting their conversation. Each pair then began its first discussion, which the experimenter started by asking subjects to turn over their cards to read the name of the topic and posing one or two questions on it, before retiring to supervise the recording process from an adjoining room.

After four minutes the experimenter returned and engaged the subjects in casual conversation for a two minute rest period before repeating the procedure for subsequent trials. At the end of the fourth discussion, the experimenter asked for any comments or questions before explaining the full purpose of the experiment. Before being dismissed, subjects were thanked for their cooperation and asked not to discuss the experiment with any other individuals still to be run.

3.1.4 Analysis and Results

Recordings were transcribed by a helper and the transcripts checked by the author, both persons carrying out their tasks ignorant of the visibility levels in the particular conversations to which they listened, although topics obviously could not be concealed. There was some muffling of sound on the recordings, caused by the curtain before the microphone, but all subjects except one provided two hundred running

words of comprehensible speech per trial and the exception, a quiet-voiced, reluctant speaker yielded more than a hundred and forty on each of the two trials for which there was a shortfall. This subject and her partner would have been replaced, but for the fact that the author moved abroad after conducting the experiment and before conducting a more than rudimentary check on the recordings themselves, but speech disturbance rates, unlike the Type-Token Ratio, are not systematically biased by the size of speech samples, so there is no need for the numbers of words in samples to be the same.

Before inferential statistics were applied, the data, in the form of filled pauses per word of speech, were normalised by Arcsine Transformation and the arcsin scores multiplied by 10^3 to avoid decimal fractions. Table 3.1 shows the outcome of the analysis of variance.

It can be seen that none of the effects remotely approached significance. The means for all levels of the Visibility, Topics and Trials factors shown in Table 3.2. were calculated from the untransformed scores and expressed as pauses per thousand words, to avoid decimals.

NonAh rates were also recorded and analysed, in case they should show any sign of variations in anxiety. The results of this analysis, which followed the same procedure as that for filled pauses, are shown in Tables 3.3 and 3.4. Barely significant at the 0.05 level, the Trials effect does not appear to indicate anything psychologically significant, although it could have been caused by a cyclical rise and fall in anxiety during the experimental sessions. None of the F ratios proved significant.

Table 3.1

Analysis of Variance for Filled Pause Rate in Experiment I*

Source	d.f.	Sum of Squares	Mean Square	F	p
Within Subjects	48	519,541			
Visibility	3	16,814	5,605	<1.0	N.S.
Topics	3	52,799	17,600	1.85	N.S.
Trials	3	52,915	17,638	1.85	N.S.
Residual	3	53,941	17,647		
Error within Ss	36	343,072	9,530		
Between Subjects	15	482,161			
Between Groups	3	81,921	27,307	<1.0	N.S.
Ss Within Groups	12	400,240	33,353		
Total	63	1,001,704			

* Unit of measurement = 10^3 x 2 arcsin $\sqrt{\text{FPs per word}}$.

Table 3.2

Filled Pause Means from Experiment I
(FPs per 1,000 words)

A. By Visibility		B. By Topic		C. By Trial	
Normal	33	Education	39	T1	40
Dark Spectacles	28	Mass Media	28	T2	33
Visor	35	Punishment	30	T3	25
Screen	29	Censorship	27	T4	27

Table 3.3

Analysis of Variance for NonAh Rate in Experiment I*

Source	d.f.	Sum of Squares	Mean Square	F	p
Within Subjects	48	260,987			
Visibility	3	17,899	5,966	1.13	N.S.
Topics	3	2,199	733	<1.00	N.S.
Trials	3	46,130	15,377	2.91	<0.05
Residual	3	4,472	1,491		
Error within Ss	36	190,287	5,285		
Between Subjects	15	497,004			
Between Groups	3	87,687	29,229	<1.0	N.S.
Ss Within Groups	12	409,317	34,110		
Total	63	755,991			

* Unit of measurement = 10^3 x 2 arcsin $\sqrt{\text{NonAhs per word}}$

Table 3.4

NonAh Means from Experiment I

(NonAhs per 1,000 words)

A. By Visibility		B. By Topic		C. By Trial	
Normal	.. 39	Education	.. 36	T1	.. 35
Dark Spectacles	.. 32	Mass Media	.. 35	T2	.. 39
Visor	.. 38	Punishment	.. 36	T3	.. 30
Screen	.. 35	Censorship	.. 38	T4	.. 40

Pearson's correlation coefficient was calculated to investigate any association between arcsin filled pause and NonAh rates and the result ($r = .145$; $n = 64$) revealed no significant relationship. Published findings of other authors who quote correlations between Ahs and NonAhs show a variety of inconsistent relationships, although they generally tend to be low and positive.

3.1.5 Discussion

This experiment offers no confirmation of the hypothesis that filled pauses are floor apportionment cues. When the positive findings on visibility and filled pause rate, by Kasl and Mahl (1965), Siegman and Pope (1968) and others, are taken into account, it is tempting to conclude that the absence of an observed effect in the present case was due to faults in the experiment, but careful adherence to procedures was maintained throughout and it is hard to see how this could be so. The least easy aspect to have confidence in is the transcription of speech, but the main problem here lay in determining which, rather than how many, words were uttered and restriction of analysis to passages of running speech makes it still less likely that any transcription errors would be enough to mask any substantial effect of the visibility manipulation.

Although the restrictions on visibility were bound to make subjects' conversations less than wholly natural, it would be easy to over-estimate this. Dark glasses are often worn indoors and the presence of a screen is by no means unrepresentative of real life: we frequently converse with people in adjoining showers, changing cubicles, library carrels or even, sometimes, lavatories. The visors, on the other hand, may have been off-putting and subjects' verbal

reports suggested that they had been slightly uncomfortable to wear, but the NonAh rate, an indicator of anxiety, was not noticeably high for this condition. Whether the NonAh rate would be expected to rise as a result of anxiety caused by the wearing of visors is open to doubt, as it is most responsive to anxiety arising from conversational content, but if in the experiment anxiety had no effect on NonAhs there is certainly no reason to suppose it would have affected Ahs enough to disguise otherwise significant differences relating to visibility.

A final possible weakness in this experiment could lie in the use of a repeated measures design, which is always bound to be open to inter-trial contamination effects. Such effects would tend to minimise differences between treatments more often than not, particularly when trials follow each other closely, but they would hardly be likely to conceal effects altogether.

It is concluded that under the conditions of Experiment I decrease in mutual visibility of persons of dyadic social interaction did not result in any increase of filled pause rate, contrary to what would be predicted from the floor control hypothesis.

3.2 EXPERIMENT II

THE EFFECTS OF VARYING PRESSURE TO TALK UPON FILLED PAUSE RATE

Aim. To establish whether pressure to maintain the floor, caused by either an interrupting interviewer or one who remains silent, increases the incidence of filled pauses.

3.2.1 Introduction

An experiment reported by Lalljee and Cook (1969) has been discussed and criticised in 2.2.2.3. Experiment II was designed to provide a similar test of the floor control hypothesis, using a slightly different procedure, and a related test, also relevant to the question of whether monologues and dialogues differ in filled pause rates.

Lalljee and Cook subjected one of two matched groups of subjects to vigorous interruption pressure on the part of a confederate with whom they discussed matters of mutual interest and disagreement, but the manipulation, in particular the instructions to the experimental group, could have caused subjects to suspect what was really going on and cease to try to hold the floor. The present author considered that an alternative manipulation, in which subjects were instructed openly that they would be subject to risk of interruption by the experimenter himself and should try to avoid losing the floor, might be superior, since there would be less doubt about subjects' beliefs concerning the experimental situation. An added possible advantage of such an approach would be that since the subject would expect, and try to avoid,

interruption, interruption pressure could in practice be manipulated less vigorously, thereby giving the subject a chance to fill his pauses and be heard.

If a speaker is expected to assert control of the floor by filling pauses when trying not to accept interruptions, it might no less be expected that a speaker attempting to maintain a stream of speech with an interlocutor who says nothing or very little would fill his pauses as an indication that he had not given up speaking himself. This prediction would exemplify a broadened interpretation of the floor apportionment hypothesis, as argued for previously. Just as in the case of interruption pressure, when his partner maintained silence, the speaker would be under pressure to continue talking and a general hypothesis could be formulated concerning differences between both experimental conditions and a control condition of 'normal' conversation free of pressure to hold the floor. If, however, filled pauses are used in floor control only to ward off interruption, it would be expected that the interruption pressure condition would be the only one showing a higher filled pause rate than the control. If, again, filled pauses are only emitted when there is strict need to apportion the floor, it might be expected that a silence treatment would show the lowest filled pause rate of all three.

The conditions under which Experiment II was conducted were less than ideal. It was carried out in Uganda and for diplomatic reasons in semi-secrecy, so shortage of native English speakers of suitable age and cultural homogeneity made another repeated measures design necessary, when the experimenter would have preferred otherwise. However, the subjects who could be recruited were extremely cooperative and, being familiar to the experimenter, were readily put at ease; they

concentrated well on the task in hand and enabled the author to establish a high degree of uniformity in his procedures, making for a completed experiment satisfactory on methodological points.

3.2.2 Experimental Design

Having repeated measures on three experimental conditions meant that three separate topics for discussion were needed and some economy of design was achieved by confounding topics with trials. All six possible orders of trials (topics) were used with one subject of each sex for each order, as male and female subjects were equally hard to obtain.

Statistically, the design selected could be regarded as a fractional replication of a 2 x 3^2 x 6 factorial experiment, with repeated measures on the two three-level factors. Fractional replications are covered by Winer (1962, pp. 447-455), but the present experiment, so considered, would be excessively complicated by the repeated measures and the fact that different factors have different numbers of levels, with one not being a prime number. The most convenient way of analysing Experiment II, it was decided, would be as a straightforward 2 x 3 (Sex x Treatments) design, with repeated measures on the second factor and six observations per cell.

The hypotheses tested in Experiment II may be stated in the following terms:

1) The filled pause rate would be higher for the Interruption condition than for the Control.
2) The filled pause rate would be either higher or lower for the Silence condition than for the Control.

3.2.3 Method

Subjects.

Six unmarried postgraduate Diploma of Education students of each sex, aged between twenty and twenty six years, were recruited from a small group just arrived from Australia and the British Isles as part of a development aid scheme. All subjects were of European race and were native English speakers, with one exception: a young woman who had been born in Estonia, had emigrated to Australia in infancy and whose English speech showed no unusual features. The sample of subjects would be pre-selected in many respects, being more intelligent and outgoing than a representative British or Australian university student sample, but it is unlikely that the students in question would show, as a group, any abnormal pattern of filled pause behaviour.

Materials.

One audio-tape recorder and two comfortable upright chairs, arranged as shown in Figure 3.2; one wristwatch for timing interviews.

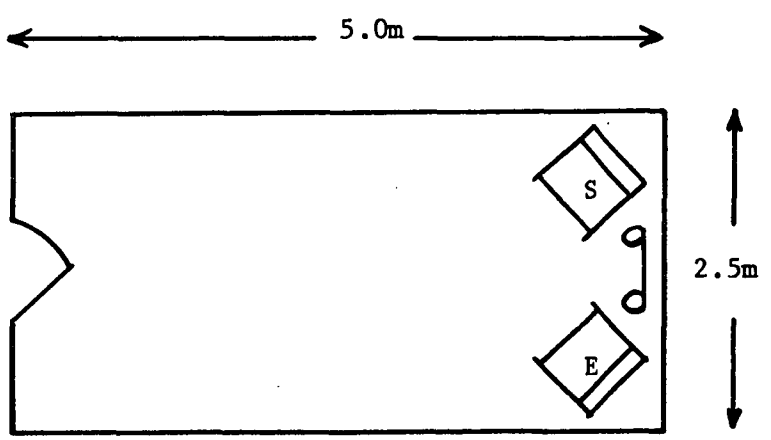

Figure 3.2 Laboratory Plan for Experiment II

Procedure.

The experimenter arranged at lectures appointments for subjects to attend three short interviews for an experiment on 'social interaction'.

Upon arrival, subject and experimenter took up positions as shown in Figure 3.2 and the experimenter explained that he wanted the subject to talk to him for five minutes about: 'What you hope to achieve in East Africa' (Trial 1); 'How Africa compares with your preconceptions' (Trial 2); 'How you expect to change as a result of your East African experience' (Trial 3). On the first trial the experimenter also explained that he would record the interviews and that he would treat them as confidential information. Subjects were next told about the experimental treatment to be applied on the trial in question, thus:

> Interruption condition: 'This time, I shall try to interrupt you if you pause. Please try to prevent me from doing so, if you can.'
>
> Silence condition: 'This time I shall not say anything once you have got started, so it is up to you to keep talking without help.'
>
> Control condition: 'This time I shall not do anything unusual in our conversation, so it will be a normal dialogue.' (This instruction was omitted for subjects who received the control treatment on Trial 1).

After checking that the subject understood what was required and answering any questions, the experimenter started the tape recorder and began the interview.

In the Control and Interruption conditions, the experimenter commented upon and asked questions supplementary to the subject's answers throughout the five minutes, attempting, in the latter condition,

to break in whenever the subject paused, though not aggressively. In practice this did not mean successful interruption on all occasions and the aim of having a less extreme manipulation of actual interruption risk than Lalljee and Cook was therefore probably achieved. In the Silence condition, the experimenter said nothing, but occasionally smiled, nodded and grunted to indicate attention, except that one subject dried up altogether and the experimenter was compelled, after nearly a minute of total silence, to ask a supplementary question. Most of the speech analysed from the interview in question occurred before the experimenter's supplementary.

At a convenient juncture after five minutes had elapsed, the experimenter asked the subject to stop and arranged for a further interview at least seven days thence before thanking and dismissing him. On the final trial, subjects were asked for comments and questions and were told the full purpose of the experiment before dismissal. Subjects were asked each time not to discuss the experiment with any other subjects until it was complete.

3.2.4 Analysis and Results

Interviews recorded in Experiment II were treated like the conversations in Experiment I, except that only the interviewee's speech was transcribed and that more words of speech were used. As the size of speech sample does not affect filled pause rate in any biased fashion, it proved more convenient to analyse a given number of lines of transcript than a set number of words and the samples varied in size from two hundred and ninety one to three hundred and fifteen words. As before, arcsin scores were used to normalise distributions and means are quoted as filled pauses per thousand words, calculated from original raw scores.

TABLE 3.5

Analysis of Variance for Experiment II*

	d.f.	Sum of Squares	Mean Square	F	p
Within Subjects	24	113,023			
Treatments	2	406	203	<1.0	N.S.
Treatments x Sex	2	11,617	5,809	1.15	N.S.
Treatment x Ss within Sexes	20	101,000	5,050		
Between Subjects	11	1,389,282			
Sex	1	127,092	127,092	1.01	N.S.
Ss within Sexes	10	1,262,190	126,219		
Total	35	1,502,305			

* Unit of measurement = $10^3 \times 2 \arcsin \sqrt{\text{FPs per word}}$

It is obvious that no significant differences emerged in Experiment II.

TABLE 3.6

Mean Filled Pause Rates from Experiment II
(FPs per 1,000 words)

A. By Treatments		B. By Sex	
Control	.. 35	Male subjects	.. 51
Interrupting Interviewer	.. 37	Female subjects	.. 21
Silent Interviewer	.. 36		

NonAh rates were not calculated for Experiment II, because the systematic differences between conditions in the experimenter's own behaviour would be likely to obscure any anxiety differences shown by subjects, through direct effects upon the subjects' speech, e.g. if a higher NonAh rate were recorded for the Interruption condition there would be no way of knowing whether this was because the subjects were more tense or because the experimenter's interruptions were breaking up their verbal planning.

3.2.5 Discussion

As with Experiment I, there is little to discuss. The results do not support the hypothesis that filled pauses have a floor control function since neither manipulation designed to increase a subject's inclination to signal possession of the floor increased filled pause rate. Although the writer experienced some difficulty in carrying the experiment out at all, it was accomplished with enough care and control to discount maladministration as an explanation of the null outcome.

Because of its magnitude, the sex difference, statistically non-significant as it is, may be noted, as its direction conforms with that observed by Feldstein et al. (1963) and, more recently, by students at the University of Tasmania (Clemes and Howarth, 1973), who measured filled pauses per unit speech time in a 'field' setting.

It had been noted by the author that subjects in Experiment II appeared to be highly consistent in their relative filled pause rates, whereas no such strong impression had been gained in Experiment I.

Spearman's rho was used to estimate the consistency of inter-subject differences in both experiments and the intercorrelations are shown in Table 3.7.

TABLE 3.7

Spearman Rank-Order Intercorrelations Between Treatments for Experiments I & II

3.7a Experiment I

	Spectacles	Visor	Screen
Normal	.691	.312	.531
Spectacles		.037	.609
Visor			.109

3.7b Experiment II

	Interruption	Silence
Control	.849	.925
Interruption		.940

Whereas the intercorrelations for Experiment I are only low to moderate, those for Experiment II are uniformally high, with no overlap between the two groups ($p < .05$, estimated by the Randomisation test for independent samples). Although the range of scores is quite similar for both experiments, the Between Subjects variations is much greater in Experiment II than Experiment I and the difference in the intercorrelations may be attributed to the fact that Experiment II subjects were more clearly distinguishable in their filled pause rates.

To a certain degree, the consistency of individual differences in both experiments attests to the reliability of the measurement processes used, although only in an indirect sense. In this connection, it is interesting to note that the lowest relationships in Experiment I were those involving the Visor condition: the treatment which is most suspected of disrupting subjects' normal social techniques.

Of the various conclusions open at this stage, one would be that Maclay and Osgood (1959) were altogether wrong in their hypothesis; another would be that filled pauses do have something to do with floor control, but that it is not affected by the kinds of manipulations attempted in Experiments I and II. Both Lalljee and Cook (1969) and the present writer have tried to raise filled pause rate by increasing risk of losing the floor and neither attempt was succeeded. An alternative approach to testing the floor apportionment hypothesis appears to be necessary and Experiments III and IV form such an attempt, using the filled pause as an independent, rather than dependent, variable and observing its effects upon the listener.

Neither increasing risk of interruption nor compelling the speaker to occupy the floor all the time, as accomplished in Experiment II, caused the speaker to utter more filled pauses, as the floor control hypothesis would predict.

CHAPTER FOUR

THE FILLED PAUSE AS AN INDEPENDENT VARIABLE

4.1 EXPERIMENT III

THE FILLED PAUSE AS A CUE IN INTERPERSONAL PERCEPTION

Aim. To determine how a speaker is perceived by listeners, when her speech contains filled pauses and other hesitations.

4.1.1 Introduction.

Experiment III was actually carried out before Experiment II, but is considered to fit more logically into the scheme of things if treated after it in this overall report. Rather than employ a hypothetic-deductive approach in the present case, when attempting to assess the cue value of the filled pause in interpersonal perception, a more Baconian attack was thought appropriate to cast the net of dependent variables as wide as subjects would be likely to tolerate, i.e., to require subjects to judge a speaker, whose filled pauses were experimentally manipulated, on as many attributes as practically possible, tapping many features of social judgement.

The materials for recording judgements used in this experiment followed the format of Osgood's Semantic Differential (Osgood, Suci and Tannenbaum, 1957), although not all of the present bipolar scales were used by Osgood. Choice of this layout was on grounds of its wide use and convenience, not because of any theoretical committment to representational mediation theory. Cross-cultural research with the Semantic Differential (Suci, 1960; Kumata and Schramm, 1956; Triandis and Osgood, 1958; Osgood, 1964; Kuusinen, 1970) has shown a moderate stability in the factorial structure of such scales and the three components of evaluation, potency and activity can be regarded as important distinctive features of affective meaning in most contexts. By sampling

widely in these three spheres, it was considered that a satisfactory likelihood of detecting any social cue functions of filled pauses would be achieved.

Subjects heard recordings of a voice which formed an example of Lambert's (1967) 'matched guise' technique, i.e., the same speaker was heard, speaking the same message, in several guises, thereby controlling a variety of possible influences upon judgements, extraneous to the experiment. The five guises employed, details of the preparation of which are to be found in 4.1.3, were:

Natural FPs (NFP), with filled pauses in natural contexts;

Unnatural FPs (UFP), with filled pauses in unlikely positions;

Pausal Phrases (PP), with redundant words and phrases (e.g. '...anyway...', '...so to speak...', '...sort of...'), interpolated into the script;

Deleted FPs (DFP), derived from NFP by deleting all filled pauses and closing the resultant gaps;

Normal (N), a fluent version of the script, with no filled pauses.

The purpose of DFP was to provide a check on the value of N as a control guise, because it is possible that filled pauses affect the sounds of surrounding words and that such contamination, rather than the filled pauses themselves, is what provides any interpersonal perceptual information which might be detected in this experiment. This was thought possible in the light of research on speech perception from the Haskins Laboratories (Liberman et al., 1967) which illustrates how adjacent phonemes contaminate each other. It is impossible to remove filled pauses from a tape recording, using the hardware available to the author, without any disruption to sound contours, and it may not be possible with more sophisticated equipment, since a basis for separating 'contamination' from what might be termed the 'legitimate shape' of the sound contours would

be extremely hard to establish. Hence which of the two control guises is more appropriate is moot and confidence of interpretation will be greatest when both differ in a similar manner from any of the other three.

The case needs to be made for distinguishing between 'Natural' and 'Unnatural' filled pauses and thence for including the latter in this experiment. Maclay and Osgood (1959), Boomer (1965) and Cook (1971) have all studied the natural distributions of filled pauses and, although the different sets of results are not wholly consistent, all point to filled pauses occurring predominantly at the beginnings of verbal units of encoding, so this provides some objective basis for a distinction between natural and unnatural placings for them in this experiment. To some extent the Maclay and Osgood finding, uncorroborated by Cook although consistent with that of Boomer, that Ahs were more common before lexical than function words, was used as a guideline and there was a final source of useful information in the form of the subjective judgements of two independent judges, both unfamiliar with research in this field, who read the script and marked where filled pauses would be most likely to occur. Since Ahs apparently are not emitted at random in spontaneous speech, it is reasonable to call those which were placed in positions fitting the criteria just mentioned (e.g., '... the um West Indian ...') 'natural FPs' and those not fulfilling those criteria (e.g., '...the West um Indian...') 'unnatural FPs'.

It was decided to include a UFP guise on the grounds that, since the floor apportionment hypothesis involves the assumption that speakers pause to plan continuations of utterances, one would expect that pausing in unusual parts of utterances would indicate abnormal thought processes. It is supposed that an average person possesses implicit sociolinguistic knowledge which he uses to interpret the speech of others

whom he hears and that this includes knowledge of when verbal planning is likely to necessitate hesitation. Someone who failed to pause when pausing appeared very likely, and who 'ummed' and 'ah-ed' when there was no apparent reason to do so, would seem to be having bizarre problems in planning his speech and convey an impression of cognitive disorder. Inclusion of the UFP guise in Experiment III provides a test of this and thus indirectly of the validity in interpersonal inference of the filled pause's hypothesised underlying cognitive basis.

There remains a rationale for the guise which has been called 'Pausal Phrases'. Redundant -- or superficially redundant -- phrases are uttered with great frequency in ordinary conversation and form the main subject of Feldman's book, Mannerisms of Speech and Gestures in Everyday Life (1959). Bernstein (1965) considers that they are characteristic of restricted codes, while Mishler and Waxler (1970) treated them as a variety of filled pause. If the latter authors were right, it would be expected that the PP guise would generate similar interpersonal judgements to the NFP and perhaps the UFP guises, in a social perception experiment so the PP guise was devised and included in the experiment in order to test this hypothesis.

4.1.2 Experimental Design

Five guises with two sexes, since subjects of both sexes were conveniently obtainable, led to the adoption of a 5 x 2, independent groups analysis of variance, applied separately to every rating scale in the experiment, with each group of subjects hearing only one guise. Administrative convenience in running large classes of subjects at a time, the occasional omission of scales by subjects and the destruction of a small amount of data during removal operations resulted in unequal

sample sizes, variable from scale to scale, ranging between a minimum of twelve in one female group for a few scales to a maximum of thirty six in one male group for most scales.

Winer (1962) offers two alternative ways of dealing with unequal sample sizes in analysis of variance, one using unweighted means and a slightly more powerful technique with fewer statistical assumptions, using Least Squares estimatation (pp. 224ff). The latter procedure was selected and a program was written in Fortran to carry it out on the ICL 1905 computer at Makerere University.

For one judgement subjects were asked to make, only categorical data could be obtained, which made Chisquare the appropriate analytical tool.

The general hypotheses to be tested, all 2-tailed, were that for each of the attributes which subjects were to judge:

1) There would be significant differences between guises (i.e. main effect for guises);
2) There would be significant differences between the judgements of male and female subjects (i.e. main effect for sex);
3) The effects for guises and sexes would not be additive (i.e. interaction of guises and sex effects).

4.1.3 Method

Subjects.

One hundred and twenty male and one hundred and thirty female first year psychology students at The Queen's University of Belfast, all naive to the full purpose of the experiment participated in large groups,

while attending laboratory classes.

Materials.

Ninetyfour 9-point bipolar scales with semantic differential format were assembled in random order, on four pages stapled together with written instructions preceding and a page of extra items following them (Appendix 4.1). Nine-point semantic differential scales have been used successfully with university students, instead of the more usual seven-point layout, by Warr and Knapper (1968) and the author himself, in unpublished work. Of the ninetyfour scales, fiftyseven were stated by Osgood et. al. (1957) to have been used by their colleagues; of fourteen others one or both poles were so mentioned by Osgood et. al.; the remaining twentythree were devised by the writer for their relevance to the experimental aims and the subjects' Ulster cultural milieu. In selecting and devising scales, direct relevance to interpersonal judgements was made paramount and metaphorical applications were avoided, a departure from Osgood's practice and theoretical stance.

Beside completing the ninetyfour 9-point scales, subjects were asked to guess the speaker's percentile rank for intelligence, her age and whether she would be expected to dominate themselves or vice versa, in conversation as well as answering some open-ended questions which were later discarded. The subjects had just been taught the concept of a percentile and it was thought that they would find the intelligence item more interesting to complete in this form than in the form of another rating scale. Age was estimated in years purely for the convenience of familiarity. The dominance judgement was in the dichotomous form chosen in order to facilitate subjects' thinking about it in concrete realistic terms.

Preparation of a script for the recordings in the experiment began with the speaker, a female assistant, talking off the cuff about a book recently read. Her words were taped, transcribed and edited to yield a 608-word passage upon which the five guises were superimposed (Appendix 4.2).

The Normal guise was prepared by recording the script as it stood; the Natural FPs guise involved interpolated filled pauses as described in 4.1.1 above; filled pauses were interpolated in unlikely positions for the Unnatural FPs guise; filled pauses were deleted from NFP to produce the Deleted FPs guise and redundant phrases were interpolated to yield the Pausal Phrases guise. All live recordings were made in a sound-damped CCTV studio at the Queen's University psychology department, DFP being recorded direct from the playback head of a second tape recorder, with the filled pauses removed by means of the Pause button of the receiving recorder. Although the hiss level was slightly greater for DFP than the non-derivative recordings, no substantial background noise could be detected on any of the tapes.

Because the script was a constant, the durations of recordings varied (Table 4.1).

TABLE 4.1

DURATIONS OF RECORDINGS

NFP	...	305	seconds,	FP rate	116	per	1,000	words
UFP	...	305	"	" "	116	"	"	"
PP	...	277	"	PP "	96*	"	"	"
DFP	...	262	"					
N	...	216	"					

* the words making up pausal phrases not included as part of the word count.

There are two reasons why variation of duration *per se* may be assumed to affect interpersonal judgements negligibly. One is that the stream of speech was fairly continuous for all guises, without lengthy silences which might give an overall impression of slowness. The other reason is that the shortest recording, being over three and a half minutes long would be sufficient for stabilisation of social perception to occur, so that extra duration alone in the range that obtained, without extra content, would not have much influence.

All guises, despite relatively profound manipulation of filled pause rate, sounded adequately natural to present to separate groups of subjects as an unprepared person speaking about her recent reading .

Procedure.

Upon assembly at their respective laboratory classes, subjects were asked to cooperate with the experimenter by participating in an investigation on social perception. The stapled booklets were distributed and subjects were requested to read the instructions and ask about anything not understood. It was explained that a tape recorded speaker would be heard over the laboratory CCTV system and that subjects could begin to make ratings as soon as transmission ceased, but not before.

After dealing with all queries, the experimenter went to the CCTV studio to transmit the recording, returning to the laboratory, where two demonstrators had remained to supervise meanwhile, at its completion. Subjects generally completed the task in ten to twelve minutes and the experimenter collected all materials before giving a brief explanation of the experiment's purpose and answering questions.

Subjects were finally asked not to discuss the experiment with any other first year psychology students until all groups had been run.

4.1.4 Analysis and Results.

Subjects' judgements on the ninetyfour semantic differential scales were scored from 1 to 9 in a left-to-right direction. For each scale, and for the intelligence and age estimates, analysis of variance was performed and when differences between guises yielded a significant F ratio individual comparisons between all guise means were conducted by the Newman-Keuls method (Winer, 1962, p.80). A conservative level of significance, 0.01, was adopted because of the large number of analyses.

Fortyeight F ratios were significant for the Guises factor, seven for the Sex factor and none for the interaction between Guises and Sex. With so many dependent variables and so much semantic overlap between them, the importance of any particular analysis of variance is less than that of the emergent overall picture, so temptation to report detailed results for each scale has been resisted. Table 4.2 on the following pages displays ordered means and F ratios for those cases in which significant effects occurred and Appendix 4.3 presents summary tables.

The dichotomous nature of subjects' judgements of the speaker's dominance relative to themselves suited Chisquare analysis. Following a significant Chisquare for the five guises, as shown in Table 4.3, individual guises were compared, using 2 x 2 tables.

TABLE 4.2

Individual Comparisons of Means, following significant F ratios in Experiment III, showing results of Newman-Keuls tests between groups, and means for males and females where the main effect for sex proved significant.

N.B. 1. All statistical decisions are at the 0.01 level of significance.

2. Underlining has been used to indicate which means are significantly different from which: two means are not significantly different if above a common line.

TABLE 4.2

Scale No.	Left-hand Pole		Means and Standard Deviations for the five guises, shown in rank order of Magnitude and labelled according to guise					Right-hand Pole	Means for Males & Females	
1	dirty	$\bar{X}$	PP 6.32	UFP 6.57	N 7.50	DFP 7.90	NFP 7.92	clean	6.84	7.56
		SD	1.92	1.77	1.57	1.25	1.34			
5	insincere	$\bar{X}$	PP 5.43	DFP 6.81	UFP 6.82	NFP 7.06	N 7.11	sincere	N.S.	
		SD	2.51	1.79	1.98	1.96	1.67			
7	dynamic	$\bar{X}$	N 4.07	DFP 5.17	UFP 5.51	PP 5.66	NFP 5.72	static	N.S.	
		SD	1.88	2.06	1.72	2.01	2.38			
8	unchaste	$\bar{X}$	PP 5.07	N 5.09	DFP 5.81	UFP 5.90	NFP 6.78	chaste	5.25	6.15
		SD	1.91	1.55	1.75	2.11	1.77			
9	intoxicated	$\bar{X}$	PP 6.79	UFP 6.96	DFP 7.83	N 7.87	NFP 8.33	sober	7.05	7.95
		SD	2.14	2.51	1.52	1.66	1.23			
10	relaxed	$\bar{X}$	N 3.89	PP 4.98	DFP 5.15	UFP 6.51	NFP 7.08	tense	N.S.	
		SD	2.45	2.66	2.79	2.01	2.05			

TABLE 4.2

Scale No.	Left-hand Pole		Means and Standard Deviations for the five guises, shown in rank order of Magnitude and labelled according to guise					Right-hand Pole	Means for Males & Females
			N	NFP	DFP	PP	UFP		
12	sane	$\bar{X}$	1.76	1.78	1.96	2.91	3.04	insane	2.66 2.01
		SD	1.24	1.37	1.10	1.78	2.46		
			N	DFP	NFP	UFP	PP		
13	industrious	$\bar{X}$	3.85	4.15	4.46	4.90	5.60	lazy	N.S.
		SD	2.04	2.05	2.02	2.32	2.02		
			UFP	PP	NFP	DFP	N		
15	awkward	$\bar{X}$	3.41	3.81	4.04	5.60	5.76	graceful	N.S.
		SD	1.78	1.94	2.01	2.02	1.80		
			PP	UFP	DFP	NFP	N		
19	unreliable	$\bar{X}$	5.05	5.55	5.60	6.14	6.57	reliable	N.S.
		SD	1.95	2.18	2.18	2.10	1.85		
			N	DFP	NFP	UFP	PP		
20	profound	$\bar{X}$	4.91	5.17	5.60	5.74	6.31	superficial	N.S.
		SD	1.88	2.35	1.90	2.19	1.89		
			DFP	NFP	UFP	PP	N		
21	delicate	$\bar{X}$	3.55	4.29	4.41	5.00	5.11	rugged	N.S.
		SD	1.64	1.44	1.81	1.64	1.89		

TABLE 4.2

Scale No.	Left-hand Pole		Means and Standard Deviations for the five guises, shown in rank order of Magnitude and labelled according to guise					Right-hand Pole	Means for Males & Females
22	humourless		NFP	UFP	DFP	PP	N	humorous	N.S.
		$\bar{X}$	5.02	5.06	5.34	6.26	6.50		
		SD	2.20	2.05	2.31	1.90	1.70		
23	feeble		UFP	NFP	PP	DFP	N	vigorous	N.S.
		$\bar{X}$	4.86	5.40	5.51	5.57	6.78		
		SD	1.97	1.64	1.87	1.79	1.33		
24	honest		UFP	DFP	N	NFP	PP	dishonest	N.S.
		$\bar{X}$	2.80	2.83	2.85	2.94	3.97		
		SD	1.60	1.72	1.57	1.57	1.93		
25	depressed		UFP	NFP	DFP	PP	N	elated	N.S.
		$\bar{X}$	4.00	4.72	4.89	4.93	6.33		
		SD	1.57	1.52	1.85	1.48	1.71		
28	defensive		UFP	DFP	NFP	PP	N	aggressive	N.S.
		$\bar{X}$	3.82	4.02	4.26	4.86	6.28		
		SD	1.59	1.94	2.01	1.81	1.05		
30	provincial		UFP	NFP	PP	DFP	N	metropolitan	N.S.
		$\bar{X}$	4.88	6.02	6.67	6.74	7.22		
		SD	2.52	2.45	2.32	2.21	1.91		

TABLE 4.2

Scale No.	Left-hand Pole		Means and Standard Deviations for the five guises, shown in rank order of Magnitude and labelled according to guise					Right-hand Pole	Means for Males & Females
31	pessimistic		NFP	PP	DFP	UFP	N	optimistic	N.S.
		$\bar{X}$	5.12	5.32	5.34	5.78	7.16		
		SD	1.84	2.07	2.26	1.89	1.33		
32	ugly		UFP	PP	NFP	N	DFP	attractive	N.S.
		$\bar{X}$	5.63	6.14	6.16	7.11	7.19		
		SD	1.75	1.93	1.54	0.89	1.27		
33	circuitous		PP	UFP	NFP	DFP	N	direct	N.S.
		$\bar{X}$	4.28	5.33	5.52	5.98	7.00		
		SD	2.66	2.60	2.36	2.32	1.82		
35	poor		PP	UFP	NFP	N	DFP	rich	N.S.
		$\bar{X}$	4.69	5.27	5.58	5.94	6.40		
		SD	1.81	1.72	1.65	1.54	1.37		
36	cautious		NFP	UFP	DFP	PP	N	rash	N.S.
		$\bar{X}$	3.40	3.71	4.28	5.24	5.44		
		SD	1.51	1.89	1.99	2.14	1.81		
38	aimless		PP	UFP	NFP	DFP	N	resolute	N.S.
		$\bar{X}$	4.67	4.92	5.66	6.02	6.44		
		SD	1.93	1.97	1.94	1.65	1.76		

TABLE 4.2

Scale No.	Left-hand Pole		Means and Standard Deviations for the five guises, shown in rank order of Magnitude and labelled according to guise					Right-hand Pole	Means for Males & Female
40	insensitive	$\bar{X}$	PP 5.83	NFP 6.66	DFP 6.72	UFP 6.76	N 7.23	sensitive	N.S.
		SD	2.20	2.00	1.88	2.01	1.44		
41	common	$\bar{X}$	PP 4.00	UFP 4.25	NFP 4.82	N 4.89	DFP 5.83	aristocratic	N.S.
		SD	1.53	1.48	1.58	1.71	1.43		
51	educated	$\bar{X}$	DFP 2.36	NFP 2.72	N 2.84	UFP 3.18	PP 4.31	ignorant	N.S.
		SD	1.06	1.75	1.88	2.05	2.20		
52	tactful	$\bar{X}$	N 3.86	DFP 3.94	NFP 3.98	UFP 4.22	PP 5.32	tactless	N.S.
		SD	1.84	2.09	2.16	1.99	1.95		
53	impure	(No significant between guises effect).						pure	5.31 6.22
57	unhealthy	$\bar{X}$	UFP 5.67	PP 6.75	N 7.26	DFP 7.30	NFP 7.42	healthy	N.S.
		SD	2.24	1.78	1.56	1.64	1.75		

TABLE 4.2

Scale No.	Left-hand Pole		Means and Standard Deviations for the five guises, shown in rank order of Magnitude and labelled according to guise					Right-hand Pole	Means for Males & Females
61	familiar	(No	significant	between	guises	effect)		strange	3.58 4.36
			UFP	PP	NFP	DFP	N		
62	bungling	$\bar{X}$	3.90	3.98	4.92	5.60	5.86	skilful	N.S.
		SD	1.76	1.74	1.91	1.86	1.56		
			NFP	DFP	UFP	PP	N		
63	abstemious	$\bar{X}$	3.88	4.04	4.82	4.88	4.97	gluttonous	N.S.
		SD	1.38	1.37	1.57	1.49	1.36		
			N	DFP	NFP	UFP	PP		
64	lucid	$\bar{X}$	3.75	4.57	4.78	5.14	5.26	obscure	N.S.
		SD	1.52	2.05	2.11	2.18	2.05		
			PP	UFP	N	DFP	NFP		
65	inattentive	$\bar{X}$	4.47	5.29	5.70	5.81	5.82	attentive	N.S.
		SD	2.11	2.43	2.26	2.35	2.19		
			NFP	DFP	N	UFP	PP		
66	polite	$\bar{X}$	2.94	3.11	3.38	3.47	4.53	impolite	N.S.
		SD	1.73	1.85	1.73	1.89	1.99		

TABLE 4.3

Scale No.	Left-hand Pole		Means and Standard Deviations for the five guises, shown in rank order of Magnitude and labelled according to guise					Right-hand Pole	Means for Males & Female
68	passive		UFP	NFP	PP	DFP	N	active	N.S.
		$\bar{X}$	4.63	5.20	5.61	5.98	6.69		
		SD	2.28	2.34	2.16	2.21	2.02		
70	orderly		DFP	NFP	N	UFP	PP	chaotic	N.S.
		$\bar{X}$	3.87	3.98	4.34	5.57	6.28		
		SD	2.20	2.19	1.88	2.14	1.78		
72	sophisticated		DFP	N	NFP	UFP	PP	unsophisticated	N.S.
		$\bar{X}$	3.83	4.07	4.88	5.80	5.83		
		SD	1.85	1.93	2.30	2.16	2.12		
75	introverted		UFP	NFP	DFP	PP	N	extraverted	N.S.
		$\bar{X}$	4.74	4.98	5.89	6.25	6.72		
		SD	2.05	2.14	1.97	2.03	1.46		
76	effortless		N	DFP	PP	NFP	UFP	laboured	N.S.
		$\bar{X}$	3.74	5.23	5.52	6.08	6.25		
		SD	1.54	2.07	2.10	2.00	1.79		
78	stale		UFP	PP	NFP	DFP	N	fresh	N.S.
		$\bar{X}$	5.31	5.62	6.02	6.36	7.05		
		SD	1.98	1.96	1.74	1.75	1.28		

TABLE 4.2

Scale No.	Left-hand Pole		Means and Standard Deviations for the five guises, shown in rank order of Magnitude and labelled according to guise					Right-hand Pole	Means for Males & Females	
81	remote		UFP	NFP	DFP	PP	N	intimate	5.89	4.91
		$\bar{X}$	4.71	4.76	5.36	5.90	6.22			
		SD	1.91	2.04	2.14	1.88	1.54			
82	quick		N	DFP	PP	NFP	UFP	slow	N.S.	
		$\bar{X}$	3.24	4.47	4.97	5.20	5.88			
		SD	1.55	2.10	2.11	2.02	2.05			
84	successful		N	DFP	NFP	PP	UFP	unsuccessful	N.S.	
		$\bar{X}$	3.25	3.70	4.00	4.26	4.79			
		SD	1.55	1.63	1.73	1.71	2.00			
86	debasing		PP	UFP	NFP	DFP	N	elevating	N.S.	
		$\bar{X}$	5.07	5.39	5.48	5.89	6.15			
		SD	1.55	1.62	1.35	1.52	1.56			
87	conservative		NFP	UFP	PP	DFP	N	progressive	N.S.	
		$\bar{X}$	4.56	5.49	5.57	5.79	6.38			
		SD	2.27	2.17	1.81	2.13	1.97			
88	heavy-smoker		PP	N	DFP	UFP	NFP	non-smoker	N.S.	
		$\bar{X}$	4.67	5.07	5.26	5.47	6.32			
		SD	1.87	1.90	2.02	2.09	2.09			

TABLE 4.2

Scale No.	Left-hand Pole		Means and Standard Deviations for the five guises, shown in rank order of Magnitude and labelled according to guise					Right-hand Pole	Means for Males & Females
			N	DFP	PP	NFP	UFP		
90	colourful	$\bar{X}$	3.24	3.96	4.40	5.08	5.49	colourless	N.S.
		SD	1.50	2.08	2.14	2.25	2.28		
			NFP	UFP	DFP	N	PP		
93	teetotal	$\bar{X}$	4.46	4.74	5.23	5.50	5.66	alcoholic	N.S.
		SD	1.91	1.94	1.63	1.36	1.58		

PERCENTILE-RANK INTELLIGENCE ESTIMATES

	PP	UFP	NFP	DFP	N
$\bar{X}$	55.0	58.9	62.0	66.5	69.5
SD	15.0	17.7	14.4	13.3	16.3

AGE ESTIMATES (YEARS)

	UFP	PP	DFP	NFP	N
$\bar{X}$	19.3	20.6	20.9	21.0	22.5
SD	2.9	2.7	2.8	2.6	4.0

TABLE 4.3

Results from Chisquare analysis for differences between guises in judged relative dominance of Subjects and Speaker, showing both actual frequencies and percentages of subject samples

	UFP	NFP	DFP	PP	N	Total
"Yourself would dominate speaker"	30 (63%)	25 (52%)	21 (49%)	18 (33%)	11 (26%)	105
"Speaker would dominate yourself"	18 (37%)	23 (48%)	22 (51%)	36 (67%)	32 (74%)	131
Total	48	48	43	54	43	236

N.B. 1. Underlining shows which pairs of guises were judged significantly differently. The differences between Normal and both Deleted and Natural FPs were significant only at the 0.05 level; the two other differences significant were so at the 0.01 level (all 2-tailed tests).

2. For the whole table, Chisquare = 16.7, df = 4, p = 0.01.

A summary of all significant differences between guises is to be found in Table 4.5 (subsection 4.1.5).

It was possible that the perceptions of certain guises would be found to be more highly correlated than those of other guises, perhaps leading on to analysis of how perceptions of guises clustered into groups, so the means for the five guises were intercorrelated across the ninetyfour rating scales, to summarise the apparent similarities of the recordings. Table 4.4 shows the intercorrelations and it can be

TABLE 4.4

Intercorrelations between means for the five guises in Experiment III, calculated across all 94 scales

(Above the diagonal shows before, and below the diagonal shows after, extraction of a general common factor by the method of simple summation [loading shown]).

	NFP	UFP	PP	DFP	N
Common factor loadings	.939	.884	.848	.940	.862
NFP		.889	.703	.892	.719
UFP	.056		.737	.739	.593
PP	-.093	-.013		.694	.663
DFP	.009	-.092	-.103		.881
N	.090	-.169	-.068	.072	

seen that the covariation is almost all accounted for by a single common factor. Taking the nature of the subjects' task into account, two elements seem to explain this: similarities between guises arising from the fact that one speaker was used for all five and subjects' shared

general notions about what humanity is like, (e.g., that people on the whole tend to be polite, rather than impolite, and clean, rather than dirty).

Extraction of a general factor might be expected to leave residual correlations which would more effectively show any important distinctions between guises, rather as Kuusinen (1970) has used inter-correlations between semantic differential scales, after partialling-out Evaluation, Potency and Activity, to investigate the Finnish implicit personality theory, but in the present case no significant covariation remains to be studied. The only suggestions to be derived from the residual intercorrelations are that the PP guise and the UFP guise each appear in contradistinction to the other guises taken as a group, but do not seem to be particularly associated with each other.

Since the general factor accounted for 80.2 per cent of the total common variance, which would preclude a latent root greater than 1.0 for any factor subsequently extracted, the intercorrelational analysis was not pursued any further.

4.1.5 Discussion

In order to facilitate comprehension and evaluation of the results, Table 4.5 has been included in this section, to afford quick reference when comparing guises, and Figure 4.1 has been prepared, to clarify the broader contrasts between guises. It is necessary to distil the results from all the judgements subjects made into as small a number of main points as possible.

TABLE 4.5

SUMMARY OF DIFFERENCES BETWEEN GUISE MEANS

FOUND SIGNIFICANT AT 0.01 LEVEL

(The polarity of some scales has been reversed for ease of interpretation, so that the left-hand pole of an entry applies more to its row than its column heading).

(LEFT-HAND POLE, AS LISTED HERE, APPLIES MORE TO ROW THAN COLUMN GUISE)

	UFP	PP	DFP	N
NFP	1. CLEAN - DIRTY 9. SOBER - INTOXICATED 12. SANE - INSANE 57. HEALTHY - UNHEALTHY	1. CLEAN - DIRTY 5. SINCERE - INSINCERE 8. CHASTE - UNCHASTE 9. SOBER - INTOXICATED 10. TENSE - RELAXED 12. SANE - INSANE 24. HONEST - DISHONEST 36. CAUTIOUS - RASH 51. EDUCATED - IGNORANT 63. ABSTEMIOUS - GLUTTONOUS 66. POLITE - IMPOLITE 70. ORDERLY - CHAOTIC 75. INTROVERTED - EXTRAVERTED 88. NON-SMOKER - HEAVY SMOKER 93. TEETOTAL - ALCOHOLIC	10. TENSE - RELAXED 15. AWKWARD - GRACEFUL 41. COMMON - ARISTOCRATIC	7. STATIC - DYNAMIC 8. CHASTE - UNCHASTE 10. TENSE - RELAXED 15. AWKWARD - GRACEFUL 22. HUMOURLESS - HUMOUROUS 23. FEEBLE - VIGOROUS 25. DEPRESSED - ELATED 28. DEFENSIVE - AGGRESSIVE 31. PESSIMISTIC - OPTIMISTIC 36. CAUTIOUS - RASH 63. ABSTEMIOUS - GLUTTONOUS 68. PASSIVE - ACTIVE 75. INTROVERTED - EXTRAVERTED 76. LABOURED - EFFORTLESS 82. SLOW - QUICK 87. CONSERVATIVE - PROGRESSIVE 90. COLOURLESS - COLOURFUL
UFP		5. SINCERE - INSINCERE 10. TENSE - RELAXED 24. HONEST - DISHONEST 30. PROVINCIAL - METROPOLITAN 36. CAUTIOUS - RASH 51. EDUCATED - IGNORANT 57. UNHEALTHY - HEALTHY 75. INTROVERTED - EXTRAVERTED DOMINANCE : UFP < PP	1. DIRTY - CLEAN 10. TENSE - RELAXED 12. INSANE - SANE 15. AWKWARD - GRACEFUL 30. PROVINCIAL - METROPOLITAN 32. UGLY - ATTRACTIVE 35. POOR - RICH 41. COMMON - ARISTOCRATIC 57. UNHEALTHY - HEALTHY 62. BUNGLING - SKILFUL 70. CHAOTIC - ORDERLY 72. UNSOPHISTICATED - SOPHISTICATED 82. SLOW - QUICK 90. COLOURLESS - COLOURFUL	1. DIRTY - CLEAN 7. STATIC - DYNAMIC 10. TENSE - RELAXED 12. INSANE - SANE 15. AWKWARD - GRACEFUL 22. HUMORLESS - HUMOUROUS 23. FEEBLE - VIGOROUS 25. DEPRESSED - ELATED 28. DEFENSIVE - AGGRESSIVE 30. PROVINCIAL - METROPOLITAN 31. PESSIMISTIC - OPTIMISTIC 32. UGLY - ATTRACTIVE 33. CIRCUITOUS - DIRECT 36. CAUTIOUS - RASH 38. AIMLESS - RESOLUTE 57. UNHEALTHY - HEALTHY 62. BUNGLING - SKILFUL 64. OBSCURE - LUCID 68. PASSIVE - ACTIVE 70. CHAOTIC - ORDERLY 75. INTROVERTED - EXTRAVERTED 76. LABOURED - EFFORTLESS 78. STALE - FRESH 81. REMOTE - INTIMATE 82. SLOW - QUICK 84. UNSUCCESSFUL - SUCCESSFUL 90. COLOURLESS - COLOURFUL I.Q. LOW - HIGH AGE YOUNG - OLD DOMINANCE UFP < N
PP			1. DIRTY - CLEAN 5. INSINCERE - SINCERE 12. INSANE - SANE 13. LAZY - INDUSTRIOUS 15. AWKWARD - GRACEFUL 21. RUGGED - DELICATE 24. DISHONEST - HONEST 33. CIRCUITOUS - DIRECT 35. POOR - RICH 38. AIMLESS - RESOLUTE 41. COMMON - ARISTOCRATIC 51. IMPOLITE - POLITE 70. CHAOTIC - ORDERLY 72. UNSOPHISTICATED - SOPHISTICATED IQ. LOW - HIGH	1. DIRTY - CLEAN 5. INSINCERE - SINCERE 7. STATIC - DYNAMIC 12. INSANE - SANE 13. LAXY - INDUSTRIOUS 15. AWKWARD - GRACEFUL 19. UNRELIABLE - RELIABLE 20. SUPERLICID - PROFOUND 23. FEEBLE - VIGOROUS 24. DISHONEST - HONEST 25. DEPRESSED - ELATED 28. DEFENSIVE - AGGRESSIVE 31. PESSIMISTIC - OPTIMISTIC 33. CIRCUITOUS - DIRECT 35. POOR - RICH 38. AIMLESS - RESOLUTE 40. INSENSITIVE - SENSITIVE 51. IGNORANT - EDUCATED 62. BUNGLING - SKILFUL 64. OBSCURE - LUCID 66. IMPOLITE - POLITE 70. CHAOTIC - ORDERLY 72. UNSOPHISTICATED - SOPHISTICATED 76. LABOURED - EFFORTLESS 78. STALE - FRESH 81. REMOTE - INTIMATE 82. SLOW - QUICK 86. DEBASING - ELEVATING IQ. LOW - HIGH
DFP				7. STATIC - DYNAMIC 21. DELICATE - RUGGED 23. FEEBLE - VIGOROUS 25. DEPRESSED - ELATED 28. DEFENSIVE - AGGRESSIVE 31. PESSIMISTIC - OPTIMISTIC 41. ARISTOCRATIC - COMMON 76. LABOURED - EFFORTLESS 82. SLOW - QUICK

4.1.5.1 Matters of Control. In comparing the Normal with the Deleted FPs guise, it is noted that most of the properties which distinguish the other three guises from DFP also distinguish them from N, although there are some which distinguish N from all four of the others, including DFP. In Figure 4.1, the only properties unique to DFP are Aristocratic and Sophisticated, all its others being shared with N. It is suggested that DFP was perceived differently from N in these two cases because of precise, 'clipped' qualitites which were caused by prosodic breaks at points where filled pauses had been removed. The true nature of this would certainly not have been transparent to the subjects.

It is apparent that DFP was judged in a fashion at least partly intermediate between N and NFP on many scales, clearly reflecting its original derivation. Conservatively, DFP might be used throughout as the safest of the control conditions, but it is preferable to take account of information from both, since the value of either as a control probably depends upon the attribute being judged. Since the main purpose of using two control conditions was to exercise caution in comparing the filled pause guises with supposedly normal speech, it is worth considering the eighteen cases in which NFP differed significantly from N: fourteen show the PP guise in the middle of the five ranked means, with NFP and UFP at one extreme and DFP and N at the other. There are only four such permutations of the five guises, out of a total of twentyfour possible rank-orderings; the probability of observing fourteen out of a sample of eighteen is well below chance, (Chisquare = 48; df - 1; $p < .001$). The extent to which the two control guises are thus mutually corroborative in contrasts with NFP and UFP enhances the

FIGURE 4.1

Figure 4.1 uses selected adjectives from poles of the scales, to illustrate representatively how each guise was judged in relation to the others. Adjectives were selected for and placed on this diagram according to two alternative criteria:

(a) those poles which were applied by Ss to one guise, significantly more than to at least three other guises on the scales in question, and

(b) in cases of scales where the guise formed two non-overlapping groups, those poles which applied to a guise or guises significantly more than two or more of the other group, are shown within the boundaries of the guise or guises concerned.

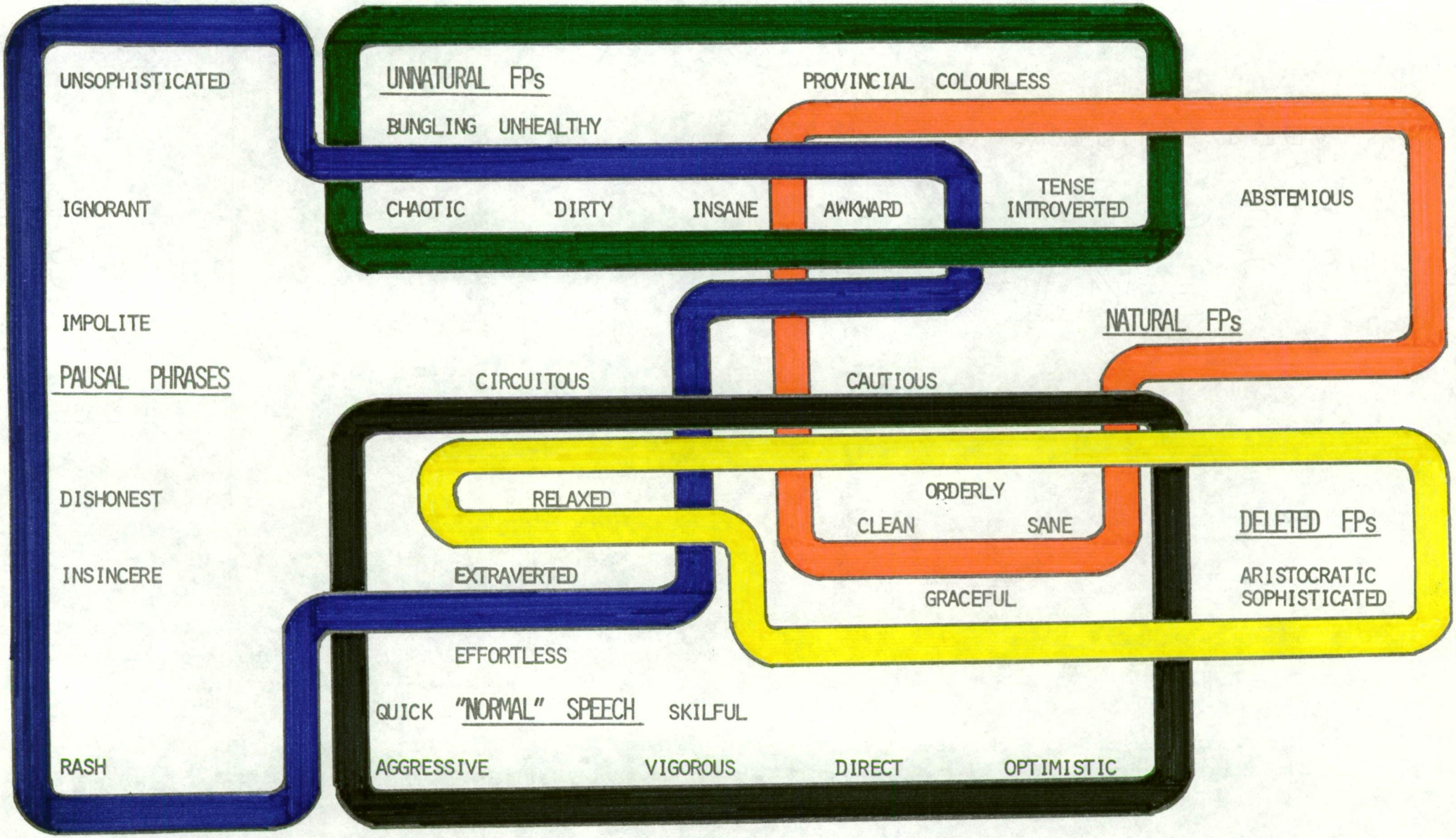

FIGURE 4.1 CHARACTERISATION OF HOW DIFFERENT VOICE GUISES WERE PERCEIVED IN EXPERIMENT IV

confidence with which they may be jointly used.

4.1.5.2 Characteristics of particular guises. The DFP condition differs significantly from N on nine scales, mostly characterising DFP as lower in health or morale, and in seven of the cases NFP also differs significantly from N. It is most likely that these latter differences between NFP and N are nothing to do with filled pauses as such.

The NFP guise itself is the focus of particular attention and there is clear indication the guise was perceived as a quieter and more anxious speaker than the N, and to a lesser extent, the DFP, guise. In Figure 4.1, Cautious and Abstemious are qualities which characterize it. Introversion is attributed to NFP, along with UFP, in some comparisons. The findings on the NFP guise are consistent with the known relation between cognitive load and emission of filled pauses, with the fact that introverts appear to reflect more than extraverts before speaking (Sandford, 1942; Ramsay, 1966) and with the relatively higher FPR for introverts observed by Siegman and Pope (1965c, 1966b). In all this may be seen weak support for the floor-apportionment hypothesis, which assumes hesitation to result from the need for time to plan speech. Again, the Eysenckian theory of introversion-extraversion (Eysenck, 1957, 1965) maintains that introverts are over- and extraverts under-socialised and it would be expected that over-socialised persons would emit more socially regulatory cues (cf. 2.2.2.4).

There are aspects of the results for NFP which are consistent with the largely discredited anxiety theory of filled pauses: NFP was perceived more as Tense than the three no-FP guises and to a certain extent as Depressed. In view of the previously reviewed data on filled pauses and anxiety, it is fair to suggest that these findings are most

likely to indicate no more than a popular stereotype of the pause-filling speaker, a stereotype which may have been the origin of the anxiety hypothesis itself.

A generalisation about NFP which the data appear to permit is that this guise did not elicit many clearly unfavourable comparisons. In some pairings it proved to be rated significantly the more Clean, Sober, Sane, Honest, Educated, Orderly and Polite, in fact. There seems to be no ground for the negative conceptions of the filled pause held by Feldman (1959), Livant (1963) and, to some extent, Goldman-Eisler (1961).

It is interesting to note that the UFP guise differed from the two controls on more, and from the PP guise on fewer, scales than NFP. This will take on more significance when PP is itself scrutinised below. In all six cases of significant differences between UFP and NFP, UFP is attributed the less desirable qualities, amounting to an easily discernible syndrome of mental inadequacy. This is further shown by the comparisons between the two control guises and UFP, to which such poles as Bungling, Obscure, Chaotic, Insane and Feeble were applied. The rationale for including an Unnatural Filled Pauses guise is justified by these results and it is possible to speculate further that filled pauses, under normal conditions, may play a part in helping the listener to follow a speaker's train of thought, by marking more clearly than a silent hesitation could the points at which he has to think most carefully. The formulation and testing of hypotheses in this area would be worthwhile, perhaps providing evidence of whether filled pauses have a useful role other than in floor-control.

Summing up the two filled pause guises together, subjects identified them fairly consistently as indicative of care and anxiety, in comparison with the fluent conditions. Such results are in agreement with either the Maclay-Osgood hypothesis or the earlier one that Ahs are elicited by anxiety. Although only at the .05 level, NFP was judged less dominant than N, like UFP, and this may be read as consistent with the Maclay-Osgood formulation, in as much as submissive persons would be more subject to risk of interruption (but see below).

The PP guise yielded a distinct picture of qualities almost uniformly undesirable. Closer scrutiny indicates an uncanny stereotype of Dr. Bernstein's working-class speaker of a restricted lingual code, with the application by subjects of the following poles, among others: Unsophisticated, Ignorant (the opposite pole was Educated), Chaotic, Common, Aimless, Obscure, Insensitive and Poor. Repetitive use of redundant phrases and 'sympathetic circularity' sequences such as '...ain't it?' is characteristic of the restricted code among the lower classes (cf. Robinson, 1972, p.155). It is possible that some of the unpleasant qualities attributed to PP may result from class stereotypes, mediated by restricted code cues to social status, but they could also have to do with a kind of cognitive functioning for which Pausal Phrases are reliable direct indicators. There is room for further enquiry on this point. Of more immediate importance, however, is that PP was apparently not perceived at all like NFP, indicating that pausal phrases are probably not a type of filled pause.

Some particular comment is appropriate with regard to the Dominance judgements made by subjects. It is clear that guises differed widely in perceived dominance, but it is not completely straightforward to interpret this in terms of the floor-control hypothesis, though it can

be seen that both guises containing Ahs were perceived as relatively submissive. The DFP guise is hard to interpret because, although it contained no filled pauses, it was judged scarcely different from the NFP guise. Whether the dominance attributed to the fluent conditions is because of their faster pace should be considered in this connection. In fact DFP (at 262 seconds) is almost exactly midway between N (216 seconds) and NFP and UFP (both 305 seconds). It would seem rather unreasonable to conclude that the same order of discrepancy in duration caused a difference between the apparent dominance of DFP and N, but not between DFP and NFP. More likely is it that aspects of the tonal pattern of NFP, probably linked with the filled pauses, but not obliterated by their deletion, conveyed information relevant to the speaker's submissiveness.

4.1.5.3 Comparisons with the results of Lay and Burron (1968). Two guises were used by Lay and Burron, a passage of very hesitant speech and an edited version of the same passage, from which all filled and silent pauses had been electronically removed, to provide a control possibly superior to the author's DFP and N. It is a pity that Lay and Burron did not distinguish filled from silent pauses. They obtained more favourable judgements for the fluent than for the hesitant guise, having previously standardised favourability norms for all their trait adjectives. By coincidence (as the author discovered the Lay and Burron paper only after Experiment III was carried out) eight of their adjectives featured as poles in the present experiment and it is possible to make comparisons. Table 4.6 shows the Lay and Burron desirability norms for these adjectives and the rank-orderings of guises in the present work. It is apparent that the N guise was judged most favourably, although the relative placings of the other four guises are not so clear.

TABLE 4.6

RANK-ORDERINGS OF MEANS FOR EIGHT SCALES,

POLES OF WHICH FEATURED AS TRAIT-NAMES IN THE EXPERIMENT OF LAY & BURRON

Scale No.	Adjective/Pole	Desirability*	Order of Guises Adj. Applies Most - Adj. Applies Least				
5	SINCERE	573	N	NFP	UFP	DFP	PP
	INTELLIGENT	537	N	DFP	NFP	UFP	PP
19	RELIABLE	527	N	NFP	DFP	UFP	PP
57	EDUCATED	500	DFP	NFP	N	UFP	PP
70	ORDERLY	399	DFP	NFP	N	UFP	PP
10	TENSE	215	NFP	UFP	DFP	PP	N
31	PESSIMISTIC	164	NFP	PP	DFP	UFP	N
13	LAZY	126	PP	UFP	NFP	DFP	N

* The desirability values are those quoted by Lay & Burron

To effect further comparison with the results of Lay and Burron, favourability indications were obtained for poles of the fiftyone rating scales which had yielded significant F ratios on the sex and/or guises factors in the present experiment, by asking fiftythree students at the University of Tasmania to tick whichever pole, if either, was the more desirable quality. The polarities submissive-dominant and intelligent-unintelligent were also included. Details are given in Appendix 4.4. For all but four of the fiftythree contrasts one pole was judged significantly more favourable than the other (Sign Test at $p = .05$). It was therefore possible to rank the guises for favourability implied by any of these scales.

Taking fortysix contrasts on which both a significant between-guises effect had been obtained in analysis of variance and one pole had been significantly judged more favourable, Friedman's Two-way Analysis of Variance by Ranks (Siegel, 1956, pp.166ff.) was applied, followed by Sign Tests for individual comparisons. Table 4.7 summarises the outcome.

TABLE 4.7

Relative Favourability of Five Speech Guises

(Results of Friedman nonparametric analysis of variance and individual Sign Tests. Non-significant differences at .05 level indicated by common underlining).

	N	DFP	NFP	UFP	PP
Rank totals (R_j)	194	171	141	95	89
Mean Ranks (high=favourable)	4.2	3.7	3.1	2.1	1.9

For Friedman's test: $N = 46$; $k = 5$; $\chi_r^2 = 74$; $p < .0005$

Both control conditions received favourable ratings relative to the two filled pause guises, but the PP guise, which it may or may not be right to regard as 'fluent' speech, received the most unfavourable judgements by far. Redundant phrases, as in the PP guise, may well have an effect upon perceived favourability quite unrelated to fluency. Clearly, the data are consistent with those of Lay and Burron, and it is possible that their results were largely determined by the Ahs in their material.

Most of the interest in the Lay and Burron results lay in sex differences and Sex x Guises interactions, which were noticeably few in Experiment III. Female subjects were more favourable to the speaker, guise notwithstanding, and provided most of the actual variation between guises, i.e., the females were more sensitive to the cues manipulated.

Examination of the significant sex differences in Experiment III does not show a consistent tendency for the females to be more generous in their perceptions, if anything, the opposite. Use of this author's own norms for favourability of the scale poles he used himself might provide a useful supplement to the significant sex differences on individual scales. However, comparison of the relative favourability of male and female judgements for the fortyseven relevant scales showed females giving more favourable ratings on twentytwo and males on twentyfive. The result does not square with that of Lay and Burron, but it must be noted that those authors did not identify their speaker's sex. It is therefore, possible that women give more favourable judgements to male, but not female, speakers.

Although, out of over ninety analyses of variance, no Sex x Guises interactions proved significant at the .01 level, it was thought worth checking to see whether, as in Lay's and Burron's experiment, females showed more sensitivity to the differences between guises than males, but to an extent statistically undetectable for any one scale. Ranges of means across the five guises were compared for male and female subjects' judgements, using the fortyeight scales on which significant between-guises effects were obtained. In thirtyone cases the females' range exceeded that of the males, in seventeen cases vice versa. The one-tailed probability of this is less than .05 (Sign Test). It is noteworthy that Lay and Burron did not present results for individual trait adjectives; their interaction effect may have been of a similar order of nagnitude to that of Experiment III and insufficient to show up on any one adjective rating.

4.1.5.4 Conclusion. In terms of the three hypotheses stated in 4.1.2 the first, concerning differences between guises, has been substantially confirmed and the differences found appear to be consistent with the hypothesis that filled pauses are involved in encounter regulation through floor-apportionment, though subjects apparently read signs of anxiety into filled pauses, as well. Very few significant sex differences were observed, however, and no Sex x Guise interactions, although there was some evidence, when large numbers of scales were taken together, that females were more responsive to guise differences than males, as Lay and Burron (1968) had found.

There are two main limiations of this experiment. One was the use of only one speaker, when it would have been preferable, but was impracticable, to have several speakers each produce all guises -- a limitation which has not previously deterred investigators from going ahead. The other is the proclivity for producing spurious differences

of laboratory experiments on interpersonal perception which compell subjects to judge target persons on the basis of impoverished evidence, for characteristics which may not be usual for individual subjects to use. The internal consistency of the results, which yielded a fairly well-rounded picture of each guise, warns against their premature dismissal on the latter ground.

4.2 EXPERIMENT IV

READINESS TO SPEAK FOLLOWING A FILLED PAUSE

Aim. To establish whether listeners pause longer before replying to a speaker's remarks when they are syntactically complete than when they are syntactically incomplete and when they are followed by a filled pause than when they are not followed by a filled pause.

4.2.1 Introduction

An utterance which ends in a grammatically incomplete sentence should suggest to the listener that its speaker has not finished and make him reluctant to start speaking immediately. If filled pauses function as Maclay and Osgood (1959) hypothesised, we might likewise expect that, placed at the ends of utterances, they too would delay the listener's assumption of the floor. Experiments reported by Cook and Lalljee (1970) were intended to test these predictions, but employed poor approximations to actual conversation, possibly because of difficulty in ensuring that the person whose utterance latencies were to be timed had anything at all to say. A suitable way of doing so is to ask the subject to conduct an interview, providing him with a schedule of topics on which to ask questions.

Such an approach was used in Experiment IV. Subjects interviewed the experimenter, who posed as a fellow-subject and terminated his answers with or without syntactical completion and with or without filled pauses. Inclusion of the syntax variable provided a check upon the appropriateness of the dependent variable, delay of onset of the subjects' speech.

4.2.2 Experimental Design

The syntax and filled pause variables provided a basic 2 x 2 analysis of variance design. It was deemed appropriate to observe each subject under all four conditions, since contamination between conditions was not anticipated. Holding order of questions constant, each of the possible twentyfour orderings of experimental conditions was used once. Since there was no interest in inter-subject differences, this confounding of orderings with subjects was acceptable.

The experimental hypotheses were:

1) Utterance latencies would be longer following syntactically incomplete remarks than syntactically complete remarks (i.e. main effect for syntax);

2) Utterance latencies would be longer following a terminal filled pause than following a remark terminated without a filled pause (i.e. main effect for filled pauses);

3) Joint occurrence of a filled pause and a grammatically incomplete sentence would yield a longer utterance latency than a completed remark without a filled pause, but that owing to redundancy of cues the filled pause and syntax effects would not summate (i.e. interaction of syntax and filled pause effects).

No predictions were made about interactions between subjects and conditions.

4.2.3 Method

Subjects. Twentyfour naive male students at the University of Tasmania,

all volunteers and aged 18 to 30 years.

Materials. Video-recording and playback equipment including zoom lens camera, two low chairs, a low table and interviewers' topic sheets.

Procedure. Each subject was recruited by an assistant, informed that he would be required to conduct or submit to a non-stressful interview and was brought to the laboratory, a carpeted room, (cf. Figure 4.2) where he was introduced to the experimenter, who was referred to as 'Pete' and wore the kind of casual attire favoured by university students. 'Pete' was asked by the assistant to be interviewee and the subject to be interviewer and both were shown to their seats, the subject being given a sheet of paper bearing the following instructions:

INSTRUCTIONS TO INTERVIEWERS

These interviews are intended to take about three to five minutes to complete. Seven general questions need to be asked, in the order given below. Please frame the questions you ask in your own words and feel free to ask supplementaries, if you wish, provided this does not make the interview too long.

Question Topic 1.
Preference for urban/rural living, giving reasons.

Question Topic 2.
Ambitions of interviewee.

Question Topic 3.
Musical preferences.

Question Topic 4.
Food preferences

Question Topic 5.
Recreational activities.

Question Topic 6.
Qualities looked for in choice of friends.

Question Topic 7.
Newspapers read regularly.

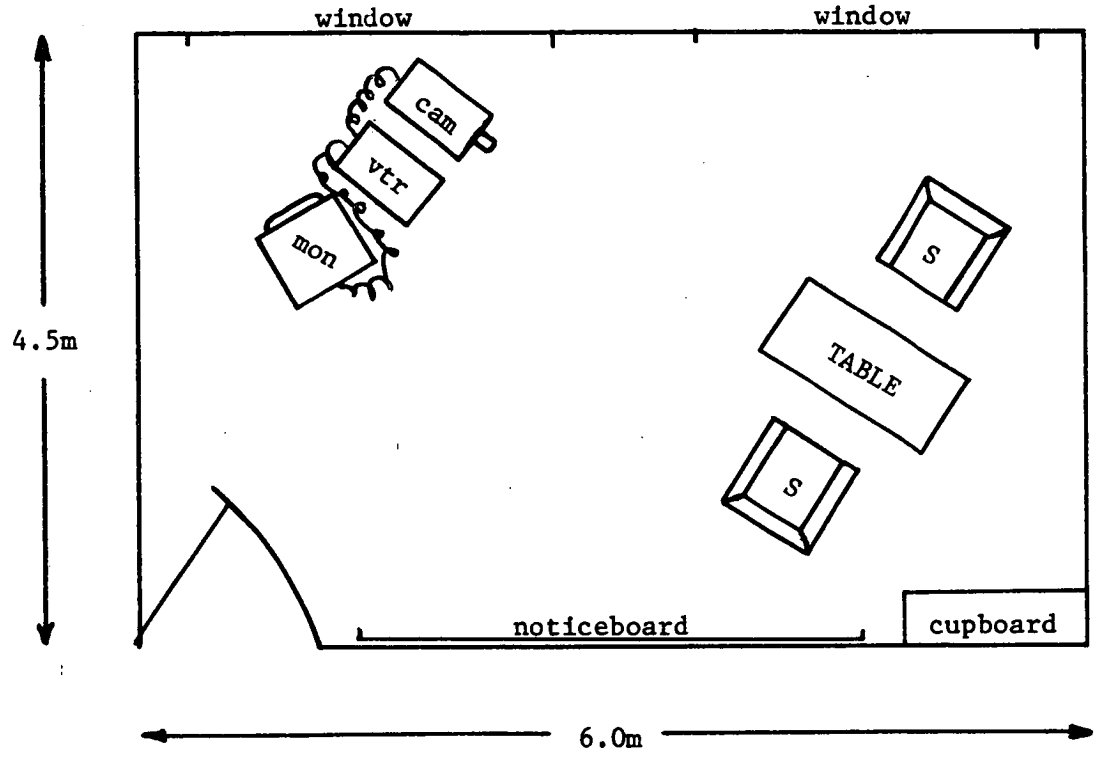

Figure 4.2 Laboratory Plan for Experiment IV

After having a few seconds to study the topic sheet, the subject was told by the assistant that he could begin the interview whenever he was ready. Most subjects responded by starting immediately.

A second assistant, manning the recording equipment, focussed the camera on the experimenter until he began to answer on the third topic of the schedule and then filmed the subject in close-up until he asked the final question, when the camera was returned to the experimenter. It was during the period that the subject was on camera that the experimental manipulations were carried out by the experimenter, the first two topics being used merely to put the subject at ease.

Answers to questions on topics 3 to 6 were terminated according to the predetermined order of conditions in the design, relevant coded details of which were attached, among other materials, to a noticeboard behind the subject and in easy view of the experimenter. Head and body movements at the ends of crucial answers were avoided by the experimenter, who directed his gaze at these junctures away from the subject, to avoid interfering floor apportionment cues.

In answering subjects' questions, the experimenter spoke from a loose script which had been derived from his off-the-cuff answers in early practice trials. Because subjects were free to frame questions in their own words, flexibility was needed in answering, but the broad content of answers remained the same for all subjects.

At the ends of interviews, subjects were given a full explanation of the purpose of the experiment and the experimenter's true identity was revealed. Under questioning, no subjects indicated any suspicion that the interviewee was not genuine or expressed any concern at the deception. Each was asked not to discuss the experiment with any other possible subjects before being dismissed.

Four subjects were replaced: one because of an order error by the experimenter, two because they covered topics in the wrong order and one because he consistently interrupted the experimenter and prevented proper administration of the procedures.

Analysis of tele-recordings. The intervals between the ends of the experimenter's answers and the subjects' next vocalisations, for answers 3 to 6 were timed with stopwatches by the experimenter and a panel of

assistants varying at different times from two to ten in number. At least one recheck was made for each instance and if the range of the panel's times was greater than 0.3 of a second further rechecking was done until a clear modal value emerged. In some cases, background noise on the sound recording, caused by body movements, and indistinctness of voice gave rise to a little difficulty in obtaining accurate times, but problems were fairly easily overcome.

4.2.4 Results and Discussion

In Table 4.8 the analysis of variance is summarised and the means of interest are shown in Table 4.9. All three experimental hypotheses are strikingly confirmed. Comparisons between the four individual means, using Tukey's (a) procedure showed that the only differences significant were those between the Syntactically Completed-No Terminal FP condition and each of the other three.

TABLE 4.8

Analysis of Variance for Experiment IV*

Source	d.f.	Sum of Squares	Mean Square	F	p
Syntax (A)	1	7,038	7,038	25.81	.001
Filled Pauses (B)	1	15,504	15,504	40.99	.001
A x B	1	9,009	9,009	19.90	.001
A x Subjects	23	6,257	272	<1.0	N.S.
B x Subjects	23	8,700	378	<1.0	N.S.
A x B x Subjects	23	10,412	453		
Between Subjects	23	23,258	1,011		
Total	95	80,178			

* Unit of measurement = 0.1 seconds.

TABLE 4.9

Means for Experiment IV in Seconds

	Syntactically Completed	Syntactically Uncompleted	Combined
Terminal Filled Pause	6.454	6.229	6.342
No Terminal FP	1.975	5.625	3.800
Combined	4.215	5.927	5.071

From these results it appears that either syntactically open sentences or sentences terminated with filled pauses, or both in combination, double delay of onset of an interlocutor's speech in the situation observed. It may need to be stressed that although this was a laboratory experiment the encounter was scripted only to a minimal degree, as far as the subject was concerned, and that the roles were such as to offer the subject, rather than the other person, apparent control of the situation. Although there would be no technical difficulties to hamper a field replication of this investigation, it would be superfluous since the original procedure was conducive to authenticity.

Although it is hard to conceive of an alternative explanation of these results than that filled pauses can indeed convey information relevant to apportionment of the floor, it may need to be borne in mind that this does not rule out other causes or functions. Moreover, the fact that they can convey such information does not necessarily mean they frequently do so, since ongoing social encounters provide several means of transmitting such cues. Nonetheless, strong support for the retention of the floor-control

hypothesis, as a contending hypothesis, is not unreasonable to draw from this study.

It is unlikely that we should, except in fairly rare cases, employ unfinished sentences to maintain the floor, although the present data imply that we could, if we wished, forestall another person's speaking by beginning a sentence we had no intent of continuing. What Goldman-Eisler (1968) has found out about speech hesitations indicates that disjointed speech is a function of cognitive load and not usually under voluntary control at all. Whether to utter an Ah is a choice about an optional extra, requiring little cognitive effort if any. Thus, the author argues that, although unfinished sentences may provide information to the listener concerning the speaker's plans for the floor, and dissuade the listener from interrupting, the information is emitted, rather than, as is more likely in the case of filled pauses, transmitted.

It is concluded that filled pauses at the ends of one speaker's remarks do tend, like grammatically unfinished sentences, to delay another speaker's beginning to speak, contrary to the verdict of Cook and Lalljee (1970) and consistent with the floor control hypothesis.

CHAPTER FIVE

GENERAL DISCUSSION AND CONCLUSIONS

5.1 The Floor Apportionment Hypothesis Reconsidered

5.2 Avenues for further Research on the Filled Pause

5.3 Social Aspects of Conversations

GENERAL DISCUSSION AND CONCLUSIONS

5.1 THE FLOOR APPORTIONMENT HYPOTHESIS RECONSIDERED

The author's four experiments have yielded results consistently neither for nor against the hypothesis that Ahs have a social regulatory role, yet on careful inspection they may be seen to caution against premature dismissal of this conjectured explanation. Experiment III and particularly Experiment IV provide positive evidence which more than outweighs the absence of support from Experiments I and II.

The floor control hypothesis was originally conceived in simple terms, as a specific account of an isolated and distinctive phenomenon. This may partly have resulted from one of its proponents being a committed S-R theorist of repute, who would tend to see behaviour in terms of single stimulus-response connections. Floor control did not even credit a mention by name in the original formulation and the filled pause was treated as a habit, a response to the speaker's own silence, reinforced by its efficacy in preventing interruption. An advantage of such a conception is that it provides no problems in explaining why filled pauses sometimes occur in monologues, but this must be almost its only advantage. As previously argued, the view that conversations are, in any significant sense, competitions for the floor is over-simple and can now be seen to be unsupported by the evidence (Lalljee and Cook, 1969; present Experiment II). Most of the time, it is suggested, floor apportionment involves striking tacit agreements over how much shall be heard from each participant, rather than the floor being up for grabs.

Both the experiment of Lalljee and Cook (1969) and the author's own Experiment II manipulated the disposition of an interlocutor to interrupt speakers and neither produced a rise in filled pause rate. If it is accepted, as would appear reasonable, that the two results are neither artefactual nor the outcomes of sloppy experimentation, it may be asked whether such experiments are really relevant to the issue at all. With hindsight it may be argued that filled pauses could not prove a barrier to a person bent upon grabbing the floor, as the confederates in both experiments would have appeared to the subjects, even though a listener uncertain that his partner had finished speaking might well be dissuaded from beginning to speak by hearing an Ah, as was demonstrated in Experiment IV. A general point may therefore be made, that experiments which manipulate perceptions of interlocutors' intent to seize the floor are unlikely to uncover associations with filled pauses, whereas experiments in which subjects' perceptions of other persons' unintentional likelihoods of interrupting are manipulated may yield relationships. Into the latter category fall the experiments of Kasl and Mahl (1965), Siegman and Pope (1968), the one sketchily reported by Mahl (1958) and the unpublished one referred to by Lalljee and Cook (1969), as well as the present Experiment I. All involved manipulations of visibility; all except the author's own experiment showed rises in filled pause rate when visibility was lowered. Such results fit well the point just advanced. It is suggested that the present failure to replicate them is attributable to the effect's sensitivity to procedural details.

By showing that filled pauses are capable of causing a person to delay substantially occupation of the conversational floor,

Experiment IV has demonstrated a means of regulating the floor, even if not that this means is normally employed by English speakers. Whether so or not, there would be nothing to prevent readers of this thesis from taking advantage of the result and in future using Ahs to assert possession of the floor in everyday life, in which case a self-fulfilling hypothesis would be an apt description.

Experiment III confirmed that listeners are not unaffected by filled pauses in a person's speech and that they draw inferences from them about ability, personality and immediate condition. In most respects, the impression listeners apparently gain from 'Ums' and 'Ahs' is consistent with the floor control hypothesis: cognitive effort, care and an undominating social persona. Listeners' impressions are not infallible guides to truth, though, and it is salutary to note that the same listeners inferred anxiety from filled pauses, although evidence against the validity of this is very persuasive.

5.2 AVENUES FOR FURTHER RESEARCH ON THE FILLED PAUSE

Further research in the same vein as the four present experiments would be appropriate, in the opinion of this writer. In using the filled pause as a dependent variable, it would generally be advisable to avoid the use of repeated measures, although in field research, when subjects are unaware of being studied at all, or when using subjects already well acquainted and less likely to infer beyond the immediate trial from partners' unusual behaviour, subjects might be used as their own controls with more chance of success.

An inevitable problem in research on the filled pause and other hesitations is that of obtaining enough of them at all to reflect changes

of independent variables. With average rates of only two or three Ahs per hundred words, large speech samples are needed, so all means must be used to raise filled pause rates to begin with, e.g., by arranging cognitively difficult verbal tasks whenever possible. The danger then needs to be heeded that filled pause rate could be boosted to a ceiling, if one were to exist, and again fail to respond to the experimental manipulations.

More experimentation with the probability of unintentional loss of floor control, varying visibility with intermediate levels, and by other means is suggested and studies of conversations with the blind and partially deaf might be suitable lines of attack. Many might be amenable to field replication, or might be carried out originally in field settings, and non-laboratory locations would be useful for cases in which the effects being studied were disrupted by the laboratory situation. In familiar, 'natural' circumstances, it is sometimes easier to demonstrate relationships, because from the subject's point of view the situation is more stable than in the laboratory.

Loss of the actual floor is only a specific example of the more general category of loss of the listener's attention and experiments would be possible in which inattention was signalled by gaze direction, body movements and trunk orientation. For example, a confederate might gaze away whenever the speaker paused or ended a sentence, to test more subtly than Experiment II the hypothesis that risk of losing the listener will increase filled pause rate. Mclean's (1969) experiment suggests the virtue of such exploration.

The possibility of restating the floor control hypothesis in more general attentional terms is something which might be considered,

as a strategy for future research. There is a recurring tendency for people, whether wearing their laymen's or scientist's hats, to be over-exclusive when testing hypotheses (Miller, 1968; Wason, 1971) and the adoption of a focussing rather than a scanning strategy (Bruner et al. 1956), by considering the filled pause as having a broad range of roles in manipulating the listener's attention would be one way of avoiding such logical pitfalls as well as accounting for their presence in monologues. The concept of Attention, moreover, has received scarcely any attention in research on social interaction and yet should prove important to understanding how interpersonal exchanges proceed (cf. Argyle and Williams, 1969, for a similar line of argument, couched in different terms). It is indeed odd that research on 'Attention' is usually seen in terms of experiments on vigilance in sonar detection, when everyday usage has much more to do with social encounter regulation.

It is possible that filled pauses have more than one distinguishable function, attentional or otherwise, within the regulatory realm or without and sub-classification of filled pauses might assist in their explanation, since the assumption that the filled pause is an indivisible unit is unwarranted and could be a source of numerous red herrings. Inconsistencies in previous results might be explicable in terms of small differences in methodology having marked effects upon relative frequencies of filled pause types. It behoves one who makes such suggestions to indicate the kind of sub-classification he has in mind and it is proposed that the most fruitful possibilities would lie in the domains of linguistic and social contexts. Linguistic context may reveal yet more about the nature of verbal planning at points where filled pauses are emitted and hence what speakers' immediate goals are. Patricia Brotherton of Melbourne University

is pursuing this approach (personal communication), with interest in whether personal feedback is involved. It might be mentioned that personal regulatory feedback itself almost suggests an underlying interpersonal role, in the past, if not at the time of emission, rather like egocentric speech in children's problem-solving. By social context is meant placement in the temporal sequence of social interaction. Although there has already been research of the kind Brotherton is doing on how filled pauses fit into verbal sequences, to the author's knowledge there has been none on sequences of social exchanges in encounters, which might concern, e.g. whether Ahs are uttered predominantly at social junctures, when possession of the floor might be most ambiguous. If social regulatory roles are hypothesised for filled pauses, there is merit in observing the social events surrounding their occurrence.

It is considered unlikely that sub-classification of Ahs in terms of the actual sound emitted will be fruitful, as this is probably governed by phonetic considerations, (e.g. we may terminate a filled pause with [m] when the following word begins with a vowel). Previous research has already implied that words and recognisable parts of words, such as repetitions and pausal phrases are not to be included in the filled pause category.

Classification of filled pauses into distinct types, if justifiable, should raise further hypotheses about separate functions: forestalling interruption, summoning attention, reassuring a listener that the reply to a question will eventually be forthcoming, etc. It is possible that the result of Experiment III, concerning unnatural placement of Ahs might prove relevant to some of these, as it appeared

that listeners might use filled pauses as cues in following the speaker's train of thought.

Even if there were no experimental evidence to suggest that filled pauses have social regulatory roles, there would be a certain logical force in the hypothesis, as it is hard to imagine any strictly linguistic or psycholinguistic basis for making sounds at all during pauses in speech. When it is recalled that speech is usually heard by someone (the speaker himself, if nobody else), possible bases begin to suggest themselves and by nature they must be socio-linguistic. It is partly because of this that the author chose to investigate the filled pause through experimental social psychology rather than linguistic distribution. Whether filled pauses are displayed by all language communities and whether there exist other, similar non-verbal vocal cues would be an interesting and relevant question and this section of the chapter will close with a relevant illustration from an East African culture.

The Bantu tribes of southern Uganda have standard greeting routines which sometimes extend over several minutes and in these, as in much everyday speech, a great deal of 'humming' is done. The most common sounds used are [m-m-m-m] and [e-e-e-e] (unfortunately, the latter sound is not necessarily distinguishable from [je-e-e-e], 'yes'). A fellow-expatriate pointed out to the author that these sounds appeared to be used to hand the floor over to another speaker. When two consecutive substantive units in the greeting sequence must be said by the same person, he waits for the go-ahead, signalled by humming, after emitting the first. Unfortunately, questioning Baganda informants on this yielded no extra insight, as they could not appreciate fully what

was being asked (i.e. whether the humming sound was a word, or just a 'noise'). It is the author's impression that the humming is strictly non-verbal and is a floor-regulating cue, comparable to the Ah in English, as viewed in the floor control hypothesis.

5.3 SOCIAL ASPECTS OF CONVERSATIONS

Regulatory cues like gaze direction and perhaps the filled pause convey no information which in principle could not be conveyed by words, but only at cost of losing the verbal channel for the primary message. For regulating encounters a small number of discrete signals, capable of speedy transmission and conveying simple messages, is needed and for this purpose non-verbal cues function well. Hierarchical systems of rules for generating temporally structured sequences of regulatory cues would be inefficient, as well as unnecessary, because they would require complex planning processes similar to that of speech itself. This is one reason for caution in pursuing the kinesic-linguistic analogy of Birdwhistell (1970).

The very nature of the floor apportionment concept presupposes that speech is fundamentally under social control and studies of regulatory cues in conversation have been typical of research on socio-linguistic relationships in being scattered and selective. A small number of floor apportionment cues have been investigated, just as a small set, from a wide variety of possibilities, of means for marking role relationships by speech have been studied. In his concluding pages, Robinson (1972) noted this unsystematic way in which sociolinguistics has begun and pointed out some of the correlations which have not been investigated, asking whether it was just the whims of individual researchers or facts of human nature which result in knowledge only of, e.g., modes of address

as relative status markers and vocalic quality as emotional indicators. It is potentially important to know the answer to this question, because if it is not investigator's whims this suggests that there exist sociolinguistic universals, comparable to universals in linguistics proper, as well as numerous biologically universal human characteristics. Such universals, if identified, would provide a paradigm for future sociolinguistic research closing the casebook on one set of questions and posing new ones about how the universals are realised in different cultural contexts.

Table 5.1 is a rudimentary attempt to map some socio-linguistic associations known to exist and to indicate, by its empty cells, conjunctions not yet, to the author's knowledge, observed in practice, although possible in principle. The table is simpler, in both its social and linguistic dimensions, than it might have been, even as drawn up by its originator, and occupied cells have not been packed with all the entries he could think of, as this would only result in confusing clutter. Choice of row and column headings is bound to be easily improved upon, but the interest does not lie in these so much as in the essential notion of a matrix of socio-linguistic dimensions. The unoccupied cells are the prime focus, because they represent potential hypotheses for future test and because those that remain, when more have been explored and in a variety of cultures, will be crucial indications of where the sociolinguistic universals, if any, are may be found.

If Table 5.1 could be improved upon; if exhaustive charting of the same sort could be achieved, it would immediately present numerous manageable tasks to researchers and theorists in the breaking

TABLE 5.1

A PROPOSED CLASSIFICATION OF SOCIO-LINGUISTIC LINKAGES

(Already reported entries in capitals; specific suggested possibilities in lower case)

		EXTRA- AND PARA-LINGUISTIC	PHONETIC AND PHONEMIC	LEXICAL	SYNTACTIC	INTER-LINGUAL	INTERMODAL
SITUATIONAL	GOALS (CONTENT)	"This is the BBC Home Service. Here is the News."		Scientific Jargon	CHOICE OF PASSIVE VOICE (Johnson-Laird, 1968)	ENGLISH-JAPANESE SWITCHING BY ISSEI BRIDES (Ervin-Tripp, 1967)	'Dear Jeremy, By the time you read this, I shall be dead...'
	INTIMACY - FORMALITY		POST-VOCALIC [r] (Labov, 1964)	MODES OF ADDRESS (Brown & Ford, 1961; Brown & Gilman, 1960)	IMPERSONAL CONSTRUCTIONS	RANAMÅL-BOKMÅL AND SPANISH-ENGLISH SWITCHING (Gumperz, 1964; Gumperz & Hernandes, 1971)	'Applications (in writing) should be addressed to..?
	CHANNEL CONSTRAINTS	FPR HIGHER UNDER LOWERED VISIBILITY (Kasl & Mahl, 1965; Siegman & Pope, 1968)	Is articulation more precise under channel restrictions?	RITUALISATION OF CONVERSATIONAL OPENINGS ON 'PHONE (Schegloff, 1968)	NO CHANGES DETECTED IN SYNTAX, UNDER REDUCED VISIBILITY (Moscovici, 1967)		LETTER-WRITING
	SPATIAL CONSTRAINTS	LOUDNESS			MORE CONNECTIVES AND NOUNS: FEWER VERBS, WITH B-B AND S-S SITTING (Moscivici, 1967)		LETTER-WRITING
IDENTIFICATIONAL	ETHNIC	N.Y. JEWISH GESTURES, (Efron, 1941, reported by Argyle, 1969)	FOREIGN ACCENTS 'velly solly'	INTERFERENCE AT LEXICAL LEVEL FROM FIRST LANGUAGE. 'he is raining'	INTERFERENCE AT SYNTACTIC LEVEL 'Mr. von Halstein very late is because he delayed by traffic was'	CANADIAN FRENCH AND ENGLISH (Lambert,1967)	
	SUB-CULTURAL	MIDDLE CLASS PAUSE MORE THAN WORKING CLASS (Bernstein 1962a)	REGIONAL ACCENTS (Giles, 1970-71) POST-VOCALIC [r] (Labov, 1964)				
	AGE			'MATEY - MATEY' REGISTER USED BY YOUNG MALE AUSTRALIANS (Kaldor, 1973)		First generation migrants who never learn language of host country.	
	SEX	MALES EMIT MORE AHs THAN FEMALES (Feldstein et.al., 1963; Clemes & Howarth, 1973)				Migrant wives who do not learn host language when husbands do at work.	
PERSONAL	ABILITY (difficult to separate from SES)	HESITATION AND EDUCATION (Feldstein et.al., 1963;Bernstein 1961a; Experiment III)				Competence in foreign languages a feature of intellectual elites (e.g. Latin)	Illiteracy
	PERSONALITY	HESITATION AND INTROVERSION (Ramsay, 1966, 1968; Experiment III)	ETHNOCENTRISM AND REACTION TO ACCENTS (Giles, 1971a)			Extraverts more willing to use foreign languages in real life situations?	Englishmen prefer written memoranda to telephone conversations (E.T. Hall)?
	ATTITUDE			"IMMEDIACY" CUES (Mehrabian, 1971)			'I bet you wouldn't put that in writing!'
	EMOTION	NONAHs AND ANXIETY (Kasl & Mahl, 1965; Cook, 1969) VOICE QUALITY (Davitz,1964) EXPERIMENT III		SUICIDE NOTES (Osgood & Walker,1959)		Reversion to one's first language, for example, swearing purposes?	
INTERPERSONAL	REGULATORY	FILLED PAUSES AND GAZE DIRECTION IN FLOOR CONTROL (Experiment IV Kendon, 1967)		'I'm sorry to butt in, but...'	UNFINISHED SENTENCES (Cook & Lalljee, 1970; Experiment IV)	Switches to another language to request feedback, check whether person still at other end of telephone line, etc.?	
	RELATIVE ROLES			MODES OF ADDRESS (Brown & Ford, 1961; Brown & Gilman,1960)		'Kisetla' and 'Kitchen Kaffir' (dialects spoken by white settlers in Africa and consisting mainly of imperatives- Scotton, personal communication)	
	ACCOMMODATION					BY USE OF OTHER'S LANGUAGE (Giles,et.al. 1972)	

down of categories along each dimension and investigation of the sub-cells thus created. Social correlates of embedding transformations might be sought, as might linguistic correlates of Intense construing on the Repertory Grid. Before the exercise degenerated into absurdity, a set of established facts should have been systematically mapped in a form which theory-builders might find congenial. For the present, needs are more modest; to define areas of ignorance in a fashion which will identify where a small amount of research may be of most value in increasing sociolinguistic understanding.

In the perspective of Table 5.1, the author's own concern until now has been with the cell defined by extralinguistic and regulatory interpersonal features. It is just one cell of the table, and would look even smaller if the table were drawn out in as full detail as presently possible, but at the same time the concern is not just with extralinguistic interpersonal regulators, still less just with 'Ers', 'Ahs' and 'Ums', but with these phenomena as pieces of a humanly significant jigsaw.

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APPENDICES

APPENDIX TO CHAPTER THREE

APPENDIX TO CHAPTER FOUR

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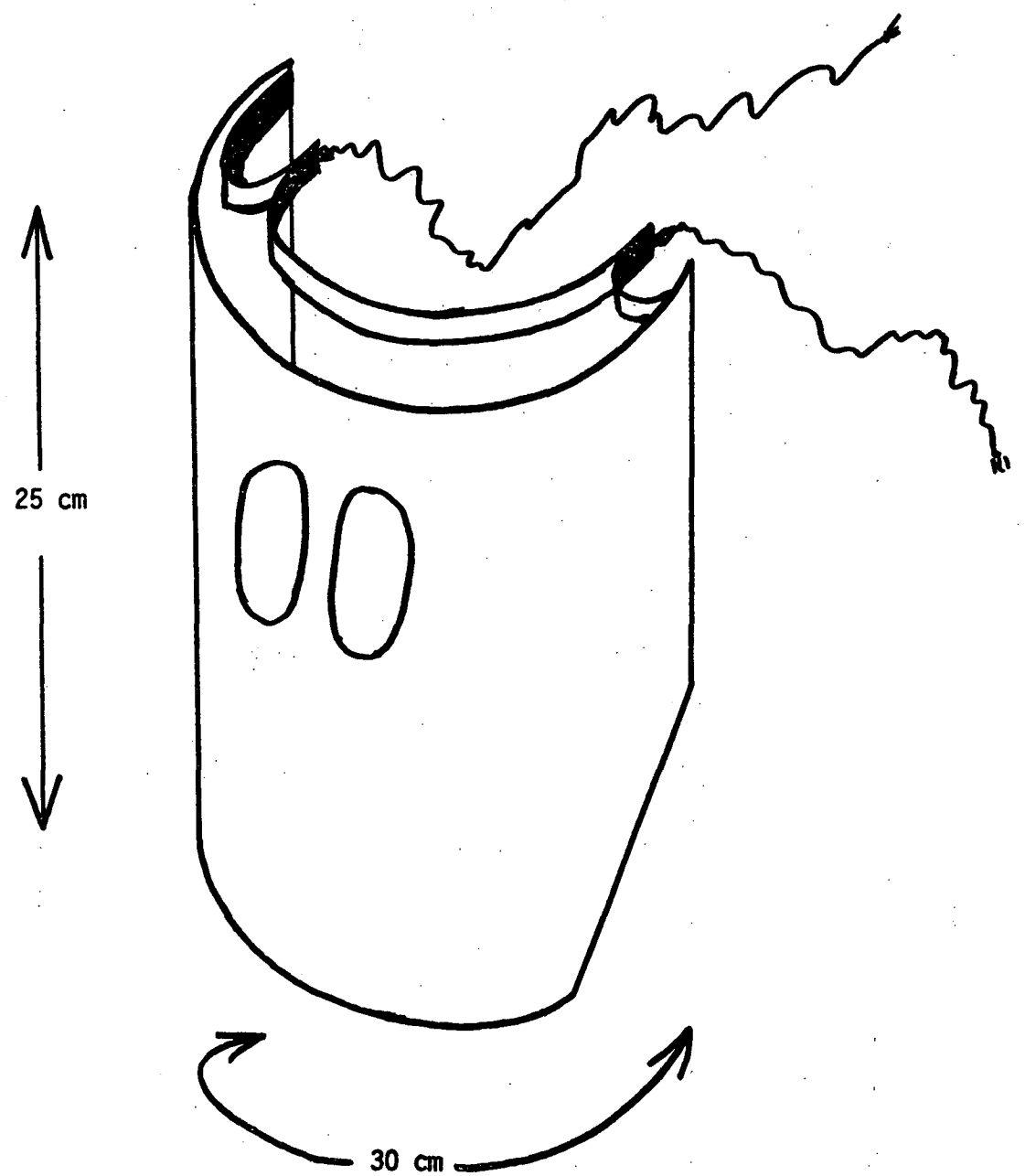
APPENDIX TO CHAPTER THREE

3.1 Visor used in Experiment I

Visors were made of cardboard and painted blue. A light spring metal strip, half an inch wide, fitted around the forehead and held the visor clear of the face. Fastening was by strings, attached to the metal strip and tying at the back of the head.



3.2 Randomised Graeco-Latin Square Design for Experiment I

Key to symbols:

A = Normal visibility
B = Dark spectacles
C = Visor + dark spectacles
D = Screen

α = Education
β = The Mass Media
γ = Punishment
δ = Censorship

Subject Groups	Trial 1	Trial 2	Trial 3	Trial 4
1	Bβ	Cα	Dδ	Aγ
2	Aδ	Dγ	Cβ	Bα
3	Cγ	Bδ	Aα	Dβ
4	Dα	Aβ	Bγ	Cδ

3.3 Instructions to Subjects in Experiment I

This experiment is concerned with the effects of mutual visibility on social interactions and I want you to hold a series of conversations about topics which I shall provide. I shall tape the four conversations and analyse them later, but the only person with access to the identities of the speakers will be myself, so talk just as freely as usual.

Altogether, it should not take more than an hour. Between conversations you may rest for a couple of minutes and at the end of your tasks you may make any comments you wish about the procedure.

During three of the trials, the amount you will be able to see of one another will be limited, by my use of dark glasses, visors and a screen. The aim of this is not to make you feel uncomfortable, so I point out that it is not really so unusual as you might imagine to find oneself talking to someone obscured, at least partly, from one's view. I hope you'll enjoy the conversations and not be put off at all.

About the actual procedure: I shall give you a topic and start you talking each time. Then, I shall leave the room, without any comment, and attend to the tape next door. All you have to do is continue talking until I return a few minutes later.

The final point concerns digressions. Don't worry if you find yourselves wandering slightly from the given topics, which are intended to provide helpful starting points, rather than unduly to restrict your scope. I would prefer you to digress a little and maintain fluid conversations than to try to stick rigidly to the set topics and run out of material altogether. All the same, if you let the topics guide the broad content of your discussions, it will help me to keep the research under scientific control.

Afterwards, I'll be able to tell you more about it all, but now, before we begin, do you want to ask any question?

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APPENDIX TO CHAPTER FOUR

4.1 Duplicated Instructions and Questionnaire used in Experiment III

(Only the first three scales are set out in full)

Key to symbols on pages one to four:

*** Scale used by Osgood et al. (1957)

** Both poles used by Osgood et al., but on separate scales

* One pole only used by Osgood et. al.

INSTRUCTIONS

You are about to hear a taperecording of a person talking. We want you to listen very carefully and to try to assess what kind of person the speaker might be. Afterwards, we want you to record your impressions, by filling the the form overleaf. DO NOT TURN OVER THE PAGE OR TRY TO EXAMINE THE FORM UNTIL INSTRUCTED TO DO SO.

Most of the items of the questionnaire are of the kind shown below, with two antonymical adjectives, one at each end of a 9-point scale. In completing these items place a cross (X) in the space which you feel best represents the speaker's qualities, in relation to the scale in question.

thin:____:____:____:____:_X_:____:____:____:____:thick

In the above example, a cross has been placed in the middle space, which you should regard as representing a "neutral" judgement, or (if you find it absolutely necessary) a judgement that the scale is quite impossible to relate to the impression you have formed of the speaker. However, it is thought unlikely that you will need to use the middle space for the latter purpose, as all of these scales can be applied in a metaphorical way, if not otherwise.

Make your judgements carefully but quickly. Previous research has shown that people are capable, after a bit of practice, of completing ten to twelve items per minute (i.e. an average time of five or six seconds per item) and that increasing the time spent does not significantly improve performance. It is your impressions which are asked for; we don't expect you already to know everything about the person you hear, but we are interested in how you guess he or she might be. Be careful not to place more than one cross on a scale.

As well as the type of item already described, there are others of a more specific nature, and which are set out differently. In each case, it is made clear, if not intrinsically obvious, what is required.

Throughout, go through the items in the order in which they are printed and do not look back to those already done, but concern yourself only with the one in hand.

Take care not to omit any items.

. . .

(I)

Give your impressions of the speaker, by checking the following scales in the way already described.

	office use only
1. dirty:___:___:___:___:___:___:___:___:___:clean	***
2. angular:___:___:___:___:___:___:___:___:___:rounded	***
3. unselfish:___:___:___:___:___:___:___:___:___:selfish	***
4. wholesome - morbid	***
5. insincere - sincere	***
6. theoretical - practical	
7. dynamic - static	***
8. unchaste - chaste	
9. intoxicated - sober	*
10. relaxed - tense	***
11. cowardly - brave	***
12. sane - insane	***
13. industrious - lazy	
14. past-oriented - future-oriented	
15. awkward - graceful	***
16. humble - proud	***
17. humane - ruthless	
18. sexless - sexy	
19. unreliable - reliable	
20. profound - superficial	***
21. delicate - rugged	***
22. humourless - humourous	*
23. feeble - vigorous	***

(2)

				office use only
24.	honest	-	dishonest	***
25.	depressed	-	elated	*
26.	hollow	-	solid	***
27.	useful	-	useless	***
28.	defensive	-	aggressive	***
29.	divergent	-	convergent	***
30.	provincial	-	metropolitan	
31.	pessimistic	-	optimistic	***
32.	ugly	-	attractive	*
33.	circuitous	-	direct	***
34.	right-wing	-	left-wing	
35.	poor	-	rich	***
36.	cautious	-	rash	***
37.	protestant	-	catholic	
38.	aimless	-	resolute	**
39.	unmarried	-	married	
40.	insensitive	-	sensitive	***
41.	common	-	aristocratic	
42.	subtle	-	obvious	***
43.	wide	-	narrow	***
44.	disloyal	-	loyal	***
45.	probable	-	improbable	***
46.	popular	-	unpopular	
47.	religious	-	irreligious	
48.	negative	-	positive	
49.	meaningful	-	meaningless	***

(3)

				office use only
50.	subjective	-	objective	***
51.	educated	-	ignorant	***
52.	tactful	-	tactless	
53.	impure	-	pure	***
54.	mild	-	intense	***
55.	yielding	-	tenacious	***
56.	unfair	-	fair	***
57.	unhealthy	-	healthy	*
58.	repentant	-	unrepentant	***
59.	open	-	closed	***
60.	inhibited	-	uninhibited	
61.	familiar	-	strange	***
62.	bungling	-	skilful	***
63.	abstemious	-	gluttonous	*
64.	lucid	-	obscure	***
65.	inattentive	-	attentive	***
66.	polite	-	impolite	
67.	central	-	peripheral	***
68.	passive	-	active	***
69.	youthful	-	mature	***
70.	orderly	-	chaotic	*
71.	sociable	-	unsociable	***
72.	sophisticated	-	unsophisticated	*
73.	masculine	-	feminine	***
74.	intuitive	-	rational	***

(4)

				office use only
75.	introverted	-	extraverted	
76.	effortless	-	laboured	*
77.	authoritarian	-	permissive	*
78.	stale	-	fresh	***
79.	cooperative	-	competitive	***
80.	urban	-	rural	***
81.	remote	-	intimate	***
82.	quick	-	slow	*
83.	cynical	-	idealistic	
84.	successful	-	unsuccessful	***
85.	orthodox	-	eccentric	**
86.	debasing	-	elevating	
87.	conservative	-	progressive	*
88.	heavy-smoker	-	non-smoker	
89.	explicit	-	implicit	***
90.	colourful	-	colourless	***
91.	gullible	-	incredulous	***
92.	complex	-	simple	***
93.	teetotal	-	alcoholic	
94.	retentive	-	forgetful	***

(5)

a) If you were introduced to the speaker, which of the two of you do you think would dominate the other in conversation?

----------yourself/speaker

b) What do you think might be the speaker's occupation?

---

c) How old do you imagine the speaker to be?_____

d) How intelligent do you imagine the speaker to be?_____
(state this as a "percentile", i.e., write down the percentage of the population who would be less intelligent than the speaker)

e) Can you make any suggestions as to what the social context of the piece of speech you heard might be?

---

f) Any further comments you may have on either the speaker or the experiment as a whole:

4.2 Script used in Experiment III, showing Placements of Interpolations

Key: Pausal Phrases underlined
* = NFP
\# = UFP

Well now,* remember* how the* Minister# of Education# banned a* book he'd# read# from schools, about a year# ago?* "To Sir,# with love"#? and all that? by* some* West# Indian author? Well, anyway,* I* read# it.

This* West# Indian,* or African, you see, comes to London, and he's a qualified engineer, or something or other, I believe; and he* tries# to get a job and all that; and when# he sends in the forms, the* firms ask# him for interview, but* of course, as soon as they see# he's coloured, so to speak, they* either say the vacancy has been filled, or* manage to make# up some other excuse for not wanting him.

Anyway, so* in# the end, he's desperate for# a job,* and he* decides, like, because there's such# a shortage of teachers and so on in London, in secondary modern schools, he'll try there.* He tries one or# two schools, anyhow, and is rejected, but eventually gets a job# at* Greenslade Secondary School, which is a very# bad school, in the* East End and all that.

Right.* He goes# along to the school, and* immediately# he enters the gate he sort of realizes what kind of a school it is; and, you know, the kind# of* discipline that is exercised there. He sees children, sitting up on# top of the* lavatory doors,* smoking, and all that sort of thing; and on his way down# the corridor to# the headmaster's# office he sees kids sort of running# out of* classrooms,* swearing,*

shouting, like.

However, somehow, he gets the job in the# end, you know. He gets the job of a man;* I can't# exactly remember his name;* but, anyway, the chap came along one# day and left at* 4 p.m. and never returned, so to speak.

Well now, his form are a leaving class; it's* a mixed# school; and altogether there are about* forty children in# his class. You know, there's a desperate battle at# first,* to get - to even - to try to get to know# them,* and all that and he finds it very difficult to - not to like them - but to get them to like him, you see, because they won't allow# him to do anything which is* pleasant, or anything.

Well, anyhow, he wins them# over in the end, by hook or by crook, and* discusses lots# of important* topics with# them,* such as* for example, colour prejudice and all that sort of thing, which is a fairly easy topic for them to talk#about, you know.

He says he'll treat them as adults and refers to# them as* Mrs. and Mrs.* and so on, - not Mrs., rather,* Miss. This,# in a sense, is quite a breakthrough, of course, and* because# of his unconventional# methods, and because he does treat# them as* grown-ups, like, they do begin# to* respond.

Well, various incidents* occur# in the book, of course; one of# them is the,* I think, one of the children in the class stabs another child, or something;* I might be# wrong about that, mind. I suppose really, the most important incident in the# book,* probably a test case for# the* class and the* teachers, so to speak, is when either a child, or a child's#

father, dies. The child# is* a half-caste, you see; his# father is a West Indian and# the* mother is an East-Ender. The teacher and the headmaster, Mr. Florian, think that he and,* if# possible,* some or other of# his class should go# along to the* funeral. Well, the teacher is quite willing to go to the funeral, naturally, but the kids are quite alarmed by this.* They're afraid, you see, of what# they* neighbours, or what the people in the area might say, and all that, if they see them going into a coloured# person's* home.

Needless to say,* the# teacher is very saddened by this, because* this is towards the end# of the school year and so on, and the children will be leaving school of course, and he's afraid that all his# work,* all his attempts at broadening the children's# minds,* so to speak, particularly in the case of* colour# prejudice,* and all that, because# they've accepted him,* you know, have all come# to nothing.* However, he goes to the funeral, anyway, and when he rounds# the corner, who does he see, sort of all standing# there* in a line waiting for him, but, of course, his own class.

oOo

No. words in PPs: c134

4.3 Summary Tables for Analyses of Variance in Experiment III

(The figures quoted do not tally exactly in most of the parts of this table, because the computer used to process the data worked to a floating decimal point and the figures from the printout have all been rounded by hand, to whole numbers, in the case of Sums of Squares, and to one decimal place, in the case of Mean Squares and F ratios)

Key: G = Guises
S = Sex
E = Error (within cells)
T = Total
* = p .01
** = p .001

	Source	d.f.	Sum of Squares	Mean Square	F
1) DIRTY-CLEAN	G	4	106	26.5	10.5**
	S	1	22	22.2	8.8*
	G x S	4	7	1.8	0.7
	E	239	602	2.5	
	T	248	738		
5) INSINCERE-SINCERE	G	4	93	23.2	5.6**
	S	1	20	19.8	4.8
	G x S	4	6	1.4	0.3
	E	240	995	4.1	
	T	249	1110		

	Source	d.f.	Sum of Squares	Mean Square	F
7) DYNAMIC-STATIC	G	4	90	22.4	5.3**
	S	1	1	1.5	0.3
	G x S	4	11	2.8	0.6
	E	240	1030	4.3	
	T	249	1130		
8) UNCHASTE-CHASTE	G	4	81	20.3	6.1**
	S	1	32	32.1	9.7*
	G x S	4	20	4.9	1.5
	E	238	786	3.3	
	T	247	919		
9) INTOXICATED-SOBER	G	4	78	19.6	5.7**
	S	1	41	40.9	11.9**
	G x S	4	21	5.2	1.5
	E	239	822	3.4	
	T	248	962		
10) RELAXED-TENSE	G	4	288	71.9	11.9**
	S	1	13	12.8	2.1
	G x S	4	8	1.9	0.3
	E	239	1440	6.0	
	T	1750			
12) SANE-INSANE	G	4	78	19.6	7.1**
	S	1	24	23.8	8.6*
	G x S	4	6	1.5	0.5
	E	240	664	2.8	
	T	249	772		

	Source	d.f.	Sum of Squares	Mean Square	F
13) INDUSTRIOUS-LAZY	G	4	96	24.0	5.5**
	S	1	2	2.5	0.6
	G x S	4	38	9.6	2.2
	E	240	1050	4.4	
	T	249	1190		
15) AWKWARD-GRACEFUL	G	4	225	56.3	14.9**
	S	1	2	2.5	0.7
	G x S	4	9	2.3	0.6
	E	240	907	3.8	
	T	249	1140		
19) UNRELIABLE-RELIABLE	G	4	69	17.1	4.0*
	S	1	2	2.1	0.5
	G x S	4	20	4.9	1.1
	E	240	1030	4.3	
	T	249	1120		
20) PROFOUND-SUPERFICIAL	G	4	61	15.3	3.6*
	S	1	1	0.7	0.1
	G x S	4	4	1.1	0.2
	E	240	1020	4.3	
	T	249	1090		
21) DELICATE-RUGGED	G	4	76	19.0	6.3**
	S	1	0	0.4	0.1
	G x S	4	1	0.4	0.1
	E	238	712	3.0	
	T	247	790		

	Source	d.f.	Sum of Squares	Mean Square	F
22) HUMOURLESS-HUMOROUS	G	4	86	21.4	5.0**
	S	1	4	4.1	1.0
	G x S	4	4	0.9	0.2
	E	240	1030	4.3	
	T	249	1130		
23) FEEBLE-VIGOROUS	G	4	93	23.3	7.5**
	S	1	0	0.0	0.0
	G x S	4	5	1.1	0.4
	E	240	745	3.1	
	T	249	843		
24) HONEST-DISHONEST	G	4	50	12.4	4.1*
	S	1	5	4.6	1.5
	G x S	4	10	2.5	0.8
	E	240	725	3.0	
	T	249	789		
25) DEPRESSED-ELATED	G	4	94	23.4	8.7**
	S	1	1	1.3	0.5
	G x S	4	2	0.4	0.2
	E	221	595	2.7	
	T	230	692		
28) DEFENSIVE-AGGRESSIVE	G	4	129	32.3	9.6**
	S	1	5	5.2	1.5
	G x S	4	7	1.7	0.5
	E	223	753	3.4	
	T	232	894		

	Source	d.f.	Sum of Squares	Mean Square	F
30) PROVINCIAL-METROPOLITAN	G	4	136	34.1	6.2**
	S	1	12	11.8	2.1
	G x S	4	13	3.2	0.6
	E	226	1250	5.5	
	T	235	1410		
31) PESSIMISTIC-OPTIMISTIC	G	4	96	23.9	5.9**
	S	1	1	1.4	0.3
	G x S	4	3	0.7	0.2
	E	223	905	4.1	
	T	232	1000		
32) UGLY-ATTRACTIVE	G	4	83	20.7	7.5**
	S	1	0	0.0	0.0
	G x S	4	4	1.0	0.4
	E	229	630	2.8	
	T	238	717		
35) POOR-RICH	G	4	82	20.5	7.4**
	S	1	1	1.5	0.5
	G x S	4	2	0.6	0.2
	E	227	633	2.8	
	T	236	719		
36) CAUTIOUS-RASH	G	4	131	32.8	9.1**
	S	1	16	16.3	4.5
	G x S	4	9	2.2	0.6
	E	228	825	3.6	
	T	237	982		

	Source	d.f.	Sum of Squares	Mean Square	F
38) AIMLESS-RESOLUTE	G	4	97	24.2	6.9**
	S	1	2	2.4	0.7
	G x S	4	26	6.5	1.8
	E	228	799	3.5	
	T	238	885		
41) COMMON-ARISTOCRATIC	G	4	97	24.2	9.9**
	S	1	2	2.0	0.8
	G x S	4	5	1.2	0.5
	E	230	562	2.4	
	T	239	665		
51) EDUCATED-IGNORANT	G	4	111	27.7	8.0**
	S	1	3	2.9	0.8
	G x S	4	5	1.2	0.3
	E	226	779	3.5	
	T	235	898		
52) TACTFUL-TACTLESS	G	4	73	18.3	4.3*
	S	1	0	0.1	0
	G x S	4	2	0.5	0.1
	E	222	946	4.3	
	T	231	1020		
53) IMPURE-PURE	G	4	36	8.9	3.2
	S	1	34	34.0	12.4**
	G x S	4	5	1.3	0.5
	E	224	616	2.8	
	T	233	691		

	Source	d.f.	Sum of Squares	Mean Square	F
57) UNHEALTHY-HEALTHY	G	4	100	25.0	7.3**
	S	1	0	0	0
	G x S	4	7	1.8	0.5
	E	228	783	3.4	
	T	237	890		
61) FAMILIAR-STRANGE	G	4	35	8.6	2.5
	S	1	27	26.9	7.7*
	G x S	4	5	1.2	0.3
	E	231	806	3.5	
	T	240	872		
62) BUNGLING-SKILFUL	G	4	151	37.6	12.3**
	S	1	10	9.6	3.1
	G x S	4	19	4.8	1.6
	E	228	696	3.1	
	T	237	875		
63) ABSTEMIOUS-GLUTTONOUS	G	4	50	12.6	5.6**
	S	1	0	0.2	0.0
	G x S	4	7	1.8	0.8
	E	230	494	2.2	
	T	239	552		
64) LUCID-OBSCURE	G	4	60	15.1	3.6*
	S	1	1	1.1	0.3
	G x S	4	18	4.6	1.1
	E	229	950	4.2	
	T	238	1030		

	Source	d.f.	Sum of Squares	Mean Square	F
65) INATTENTIVE-ATTENTIVE	G	4	72	17.9	3.4*
	S	1	2	2.5	0.5
	G x S	4	22	5.5	1.0
	E	230	1210	5.3	
	T	239	1300		
66) POLITE-IMPOLITE	G	4	79	19.9	5.6**
	S	1	0	0.4	0.1
	G x S	4	10	2.5	0.7
	E	230	816	3.6	
	T	239	906		
68) PASSIVE-ACTIVE	G	4	106	26.4	5.5**
	S	1	0	0.0	0.0
	G x S	4	18	4.6	0.9
	E	232	1120	4.8	
	T	241	1240		
70) ORDERLY-CHAOTIC	G	4	240	60.0	14.2**
	S	1	6	6.0	1.4
	G x S	4	22	5.5	1.3
	E	231	976	4.2	
	T	240	1240		
72) SOPHISTICATED-UNSOPHISTICATED	G	4	173	43.3	9.8**
	S	1	4	3.8	0.9
	G x S	4	2	0.5	0.1
	E	236	1040	4.4	
	T	245	1220		

	Source	d.f.	Sum of Squares	Mean Square	F
75) INTROVERTED-EXTRAVERTED	G	4	128	32.1	8.4**
	S	1	1	0.6	0.2
	G x S	4	15	3.7	1.0
	E	231	882	3.8	
	T	240	1030		
76) EFFORTLESS-LABOURED	G	4	155	38.7	10.0**
	S	1	0	0.2	0.1
	G x S	4	8	2.1	0.5
	E	229	884	3.9	
	T	238	1050		
78) STALE-FRESH	G	4	83	20.8	6.3**
	S	1	2	1.9	0.6
	G x S	4	4	0.9	0.3
	E	234	770	3.3	
	T	243	859		
81) REMOTE-INTIMATE	G	4	65	16.2	4.5*
	S	1	38	37.7	10.4*
	G x S	4	16	4.1	1.1
	E	235	850	3.6	
	T	245	1150		
84) SUCCESSFUL-UNSUCCESSFUL	G	4	61	15.2	5.0**
	S	1	0	0.3	0.1
	G x S	4	30	7.4	2.5
	E	234	705	3.0	
	T	243	798		

	Source	d.f.	Sum of Squares	Mean Square	F
86) DEBASING-ELEVATING	G	4	36	8.9	3.8*
	S	1	3	2.6	1.1
	G x S	1	5	1.3	0.5
	E	233	552	2.4	
	T	242	595		
87) CONSERVATIVE-PROGRESSIVE	G	4	76	18.9	4.3*
	S	1	11	10.8	2.5
	G x S	4	15	3.6	0.8
	E	236	1030	4.4	
	T	245	1130		
88) HEAVY SMOKER-NON-SMOKER	G	4	87	21.9	5.4**
	S	1	16	16.3	4.0
	G x S	4	12	2.9	0.7
	E	236	952	4.0	
	T	245	1070		
90) COLOURFUL-COLOURLESS	G	4	137	34.3	7.9**
	S	1	4	4.4	1.0
	G x S	4	19	4.6	1.1
	E	235	1020	4.4	
	T	244	1180		
93) TEETOTAL-ALCOHOLIC	G	4	56	13.9	4.7*
	S	1	4	4.5	1.5
	G x S	4	14	3.4	1.2
	E	234	691	3.0	
	T	243	765		

	Source	d.f.	Sum of Squares	Mean Squares	F
INTELLIGENCE PERCENTILE ESTIMATES	Between Guises	4	7,044	1,761	7.2*
	Within Guises	247	60,101	243	
		251	67,145		
AGE ESTIMATES	Between Guises	4	255	64	6.8*
	Within Guises	240	2,241		
		245	2,496		

4.4 Desirability Norms on Fiftythree Bipolar Contrasts showing Norms obtained.

Obtained from judgements of 53 subjects.

Key: N = No. of Ss responding to item

f = No. of Ss ticking pole concerned

$\% = \frac{100f}{53}$

Underlining indicates pole judged significantly more desirable ($p < .05$)

N	f		%		f		%
29	6	submissive	11	-	23	dominant	43
50	49	intelligent	92	-	1	unintelligent	2
50	0	dirty	0	-	50	clean	94
52	0	insincere	0	-	52	sincere	98
47	46	dynamic	87	-	1	static	2
36	13	unchaste	25	-	23	chaste	43
44	2	intoxicated	4	-	42	sober	79
47	46	relaxed	87	-	1	tense	2
47	45	sane	85	-	2	insane	4
47	43	industrious	81	-	4	lazy	8
47	2	awkward	4	-	45	graceful	85
53	0	unreliable	0	-	53	reliable	100
47	47	profound	89	-	0	superficial	0
36	17	delicate	32	-	19	rugged	36
53	0	humourless	0	-	53	humourous	100
45	0	feeble	0	-	45	vigorous	85
51	51	honest	96	-	0	dishonest	0
43	0	depressed	0	-	43	elated	81
27	10	defensive	19	-	17	aggressive	32
33	25	provincial	47	-	8	metropolitan	15
46	0	pessimistic	0	-	46	optimistic	87
47	2	ugly	4	-	45	attractive	85
47	1	circuitous	2	-	46	direct	87
36	6	poor	11	-	30	rich	57

N	f		%		f		%
39	36	cautious	68	-	3	rash	6
47	0	aimless	0	-	47	resolute	89
49	0	insensitive	0	-	49	sensitive	92
33	17	common	32	-	16	aristocratic	30
49	48	educated	91	-	1	ignorant	2
51	49	tactful	92	-	2	tactless	4
39	4	impure	8	-	35	pure	66
53	0	unhealthy	0	-	53	healthy	100
34	24	familiar	45	-	10	strange	19
47	0	bungling	0	-	47	skilful	89
33	33	abstemious	62	-	0	gluttonous	0
43	39	lucid	74	-	4	obscure	8
49	0	inattentive	0	-	49	attentive	92
51	51	polite	96	-	0	impolite	0
42	1	passive	2	-	41	active	77
39	35	orderly	66	-	4	chaotic	8
41	31	sophisticated	38	-	10	unsophisticated	19
29	2	introverted	4	-	27	extraverted	51
42	36	effortless	68	-	6	laboured	11
51	1	stale	2	-	50	fresh	94
34	6	urban	11	-	28	rural	53
45	0	remote	0	-	45	intimate	85
34	31	quick	58	-	3	slow	6
44	43	successful	81	-	1	unsuccessful	2
43	0	debasing	0	-	43	elevating	81
38	2	conservative	4	-	36	progressive	68
45	0	heavy-smoker	0	-	45	non-smoker	85
48	47	colourful	89	-	1	colourless	2
27	22	teetotal	42	-	5	alcoholic	9